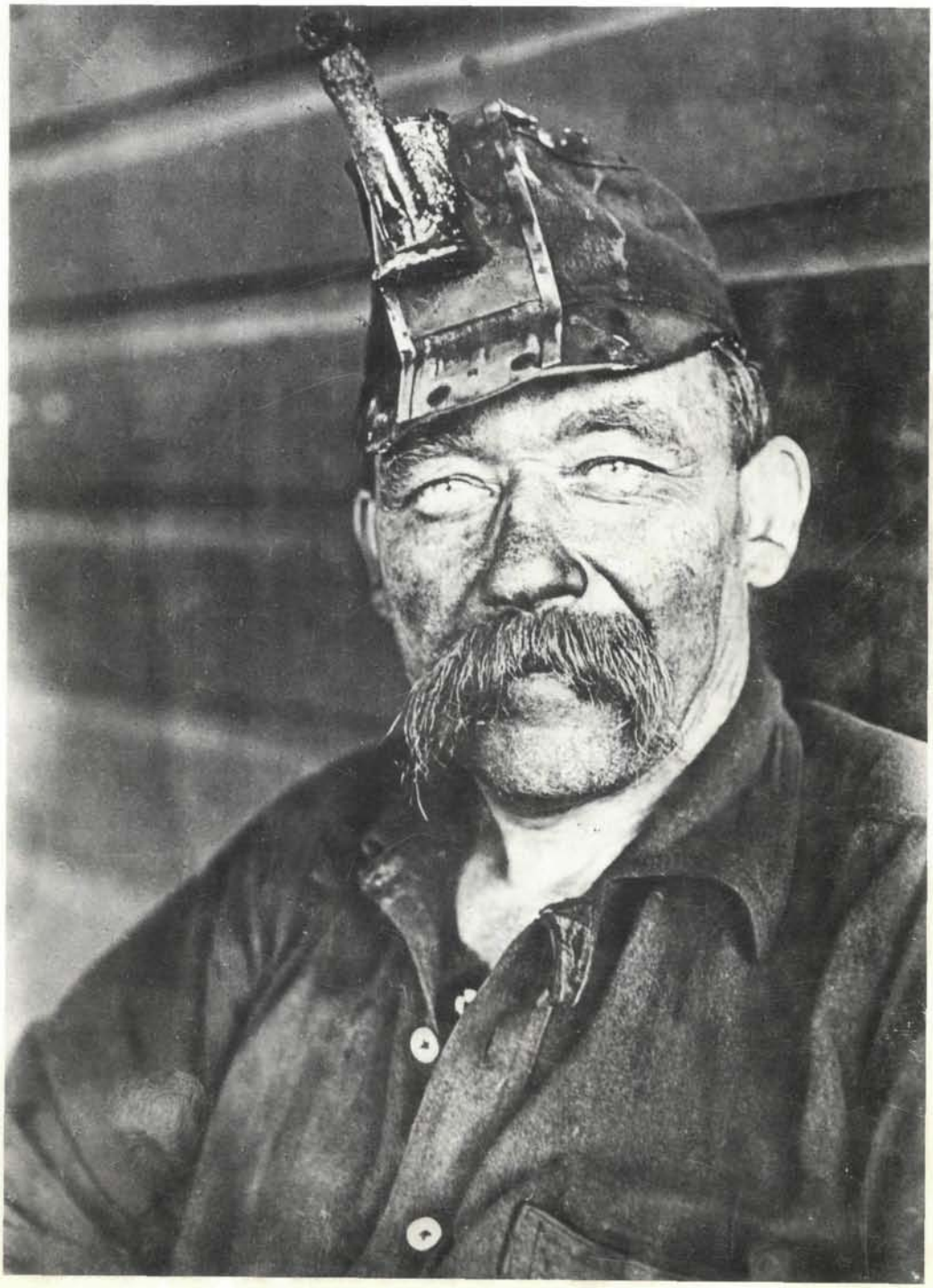
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Work, Culture, and Society in Industrializing America, 1815-1919

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THE WORK ETHIC remains a central theme in the American experience, and to study this subject afresh means to re-examine much that has been assumed as given in the writing of American working-class and social history. Such study, moreover, casts new light on yet other aspects of the larger American experience that are usually not associated with the study of ordinary working men and women. Until quite recently, few historians questioned as fact the ease with which most past Americans affirmed the "Protestant" work ethic.[1] Persons much more prestigious and influential than mere historians have regularly praised the powerful historical presence of such an ethic in the national culture. A single recent example suffices. In celebrating Labor Day in 1971, the nation's president saluted "the dignity of work, the value of achievement, [and] the morality of self-reliance. None of these," he affirmed, "is going out of style." And yet he worried somewhat. "Let us also recognize," he admitted, "that the work ethic in America is undergoing some changes."[2] The tone of his concern strongly suggested that it had never changed before and even that men like Henry Ford and F. O. Taylor had been among the signers of the Mayflower Compact or, better still, the Declaration of Independence.

It was never that simple. At all times in American history—when the country was still a preindustrial society, while it industrialized, and after it had become the world's leading industrial nation—quite diverse Americans, some of them more prominent and powerful than others, made it

Earlier versions of this paper were delivered at the Anglo-American Colloquium in Labour History sponsored by the Society for the Study of Labour History in London, June 1968; and at the meeting of the Organization of American Historians in Philadelphia, April 1969. Several friends and colleagues made incisive and constructive criticisms of these drafts, and I am in their debt: Eric Foner, Gregory S. Kealey, Christopher Lasch, Val Lorwin, Stephan Thernstrom, Alfred F. Young, and especially Neil Harris and Joan Wallach Scott. So, too, it has profited much from comments by graduate seminar students at the University of Rochester. My great debt to E. P. Thompson should be clear to those who even merely skim these pages.

[1] See especially the splendid essays by Edmund S. Morgan, "The Labor Problem at Jamestown, 1607–18," *AHR*, 76 (1971): 595–611, and C. Vann Woodward, "The Southern Ethic in a Puritan World," in his *American Counterpoint, Slavery and Racism in the North-South Dialogue* (Boston, 1971), 13–46.

[2] Quoted in the New York *Times*, Apr. 2, 1972.

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clear in their thought and behavior that the Protestant work ethic was not deeply engrained in the nation's social fabric. Some merely noticed its absence, others advocated its imposition, and still others represented an entirely different work ethic. During the War of Independence a British manufacturer admitted that the disloyal colonists had among them many "good workmen from the several countries of Europe" but insisted that the colonists needed much more to develop successful manufactures. "It is not enough that a few, or even a great number of people, understand manufactures," he said; "the spirit of manufacturing must become the general spirit of the nation, and be incorporated, as it were, into their very essence. . . . It requires a long time before the personal, and a still longer time, before the national, habits are formed." This Englishman had a point. Even in the land of Benjamin Franklin, Andrew Carnegie, and Henry Ford, nonindustrial cultures and work habits regularly thrived and were nourished by new workers alien to the "Protestant" work ethic. It was John Adams, not Max Weber, who claimed that "manufactures cannot live, much less thrive, without honor, fidelity, punctuality, and private faith, a sacred respect for property, and the moral obligations of promises and contracts." Only a "decisive, as well as an intelligent and honest, government," Adams believed, could develop such "virtues" and "habits." Others among the Founding Fathers worried about the absence of such virtues within the laboring classes. When Alexander Hamilton proposed his grand scheme to industrialize the young republic, an intimate commented, "Unless God should send us saints for workmen and angels to conduct them, there is the greatest reason to fear for the success of the plan." Benjamin Franklin shared such fears. He condemned poor relief in 1768 and lamented the absence among contemporaries of regular work habits. "Saint *Monday,*" he said, "is as duly kept by our working people as *Sunday;* the only difference is that instead of employing their time cheaply at church they are wasting it expensively at the ale house." Franklin believed that if poorhouses shut down "Saint Monday and Saint Tuesday" would "soon cease to be holidays."[3]

Franklin's worries should not surprise us. The Founding Fathers, after all, lived in a preindustrial, not simply an "agrarian" society, and the prevalence of premodern work habits among their contemporaries was natural. What matters here, however, is that Benjamin Franklin's ghost haunted later generations of Americans. Just before the First World War the International Harvester Corporation, converted to "scientific

[3] "A Manufacturer," London *Chronicle,* Mar. 17, 1778, quoted in *Pennsylvania Magazine of History and Biography,* 7 (1883): 198–99. John Adams to Tench Coxe, May 1792, quoted in *National Magazine,* 2 (1800): 253–54, in Joseph Davis, *Essays in the Earlier History of the American Corporation* (New York, 1917) 1: 500; Thomas Marshall? to Alexander Hamilton, Sept./Oct. 1971, in Harold C. Syrett, ed., *The Papers of Alexander Hamilton,* 9 (New York, 1965): 250–52; Benjamin Franklin, *Writings, 1767–1772,* ed. A. H. Smith (New York, 1907), 5: 122–27, 534–39.

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management" and "welfare capitalism," prepared a brochure to teach its Polish common laborers the English language; "Lesson One," entitled "General," read:

> I hear the whistle. I must hurry.
> I hear the five minute whistle.
> It is time to go into the shop.
> I take my check from the gate board and hang it
> on the department board.
> I change my clothes and get ready to work.
> The starting whistle blows.
> I eat my lunch.
> It is forbidden to eat until then.
> The whistle blows at five minutes of starting time.
> I get ready to go to work.
> I work until the whistle blows to quit.
> I leave my place nice and clean.
> I put all my clothes in the locker.
> I must go home.

This document illustrates a great deal. That it shows the debasement of the English language, a process closely related to the changing ethnic composition of the American working population and the social need for simplified English commands, is a subject for another study. Our immediate interest is in the relationship it implies between Americanization, factory work habits, and improved labor efficiency.[4]

Nearly a century and a half separated the International Harvester Corporation from Benjamin Franklin, but both wanted to reshape the work habits of others about them. Machines required that men and women adapt older work routines to new necessities and strained those wedded

[4] Gerd Korman, "Americanization at the Factory Gate," *Industrial and Labor Relations Review*, 18 (1965): 402. See also his *Industrialization, Immigrants, and Americanization: The View from Milwaukee* (Madison, 1967). These instructions should be compared to those issued in February 1971 by LaGrange, Illinois, General Motors officials to engine division supervisory personnel: "BELL TO BELL POLICY: It is the policy of the [electomotive] division that all employe[e]s be given work assignments such that all will be working effectively and efficiently during their scheduled working hours except for the time required for allowable personal considerations. EACH EMPLOYEE WILL BE INSTRUCTED ON THE FOLLOWING POINTS: 1. Be at their work assignment at the start of the shift. 2. Be at their work assignment at the conclusion of their lunch period. 3. All employe[e]s will be working effectively and efficiently until the bell of their scheduled lunch period and at the end of their scheduled shift. 4. Employe[e]s are to work uninterrupted to the end of the scheduled shift. In most instances, machines and area clean-up can be accomplished during periods of interrupted production prior to the last full hour of the shift." These instructions came to my attention after I read an earlier version of this paper to students and faculty at Northern Illinois University. Edward Jennings, a student and a member of Local 719, United Automobile Workers, delivered the document to me the following day. See also the copy of the work rules posted in 1888 in the Abbot-Downing Factory in Concord, New Hampshire, and deposited in the New Hampshire Historical Society. Headed "NOTICE! TIME IS MONEY!" the rules included the following factory edict: "There are conveniences for washing, but it must be done outside of working hours, and not at our expense." I am indebted to Harry Scheiber for bringing this document to my attention.

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to premodern patterns of labor. Half a century separated similar popular laments about the impact of the machine on traditional patterns of labor. In 1873 the Chicago *Workingman's Advocate* published "The Sewing Machine," a poem in which the author scorned Elias Howe's invention by comparing it to his wife:

> Mine is not one of those stupid affairs
> That stands in the corner with what-nots and chairs . . .
> Mine is one of the kind to love,
> And wears a shawl and a soft kid glove . . .
> None of your patent machines for me,
> Unless Dame Nature's the patentee!
> I like the sort that can laugh and talk,
> And take my arm for an evening walk;
> And will do whatever the owner may choose,
> With the slightest perceptible turn of the screws.
> One that can dance—and possibly flirt—
> And make a pudding as well as a shirt;
> One that can sing without dropping a stitch,
> And play the housewife, lady, and witch . . .
> What do you think of my machine,
> Ain't it the best that ever was seen?
> 'Tisn't a clumsy, mechanical toy,
> But flesh and blood! Hear that my boy.

Fifty years later, when significant numbers of Mexicans lived in Chicago and its industrial suburbs and labored in its railroad yards, packing houses, and steel mills (in 1926, thirty-five per cent of Chicago Inland Steel's labor force had come from Mexico), "El Enganchado" ("The Hooked One"), a popular Spanish tune, celebrated the disappointments of immigrant factory workers:

> I came under contract from Lorelia.
> To earn dollars was my dream,
> I bought shoes and I bought a hat
> And even put on trousers.
> For they told me that here the dollars
> Were scattered about in heaps
> That there were girls and theatres
> And that here everything was fun.
> And now I'm overwhelmed—
> I am a shoemaker by trade
> But here they say I'm a camel
> And good only for pick and shovel.
> What good is it to know my trade
> If there are manufacturers by the score
> And while I make two little shoes
> They turn out more than a million?
> Many Mexicans don't care to speak
> The language their mothers taught them

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And go about saying they are Spanish
And denying their country's flag . . .
My kids speak perfect English
And have no use for Spanish,
They call me "fadder" and don't work
And are crazy about the Charleston.
I am tired of all this nonsense
I'm going back to Michogan.

American society differed greatly in each of the periods when these documents were written. Franklin personified the successful preindustrial American artisan. The "sewing girl" lived through the decades that witnessed the transformation of preindustrial into industrial America. Harvester proved the nation's world-wide industrial supremacy before the First World War. The Mexican song served as an ethnic Jazz Age pop tune. A significant strand, however, tied these four documents together. And in unraveling that strand at particular moments in the nation's history between 1815 and 1920, a good deal is learned about recurrent tensions over work habits that shaped the national experience.[5]

The traditional imperial boundaries (a function, perhaps, of the professional subdivision of labor) that have fixed the territory open to American labor historians for exploration have closed off to them the study of such important subjects as changing work habits and the culture of work. Neither the questions American labor historians usually ask nor the methods they use encourage such inquiry. With a few significant exceptions, for more than half a century American labor history has continued to reflect both the strengths and the weaknesses of the conceptual scheme sketched by its founding fathers, John R. Commons and others of the so-called Wisconsin school of labor history.[6] Even their most severe critics, including the orthodox "Marxist" labor historians of the 1930s, 1940s, and 1950s and the few New Left historians who have devoted attention to American labor history, rarely questioned that conceptual

[5] "The Sewing Machine," *Workingman's Advocate* (Chicago), Aug. 23, 1873; "El Enganchado," printed in Paul Taylor, *Mexican Labor in the United States: Chicago and the Calumet Region* (Berkeley, 1932), vi–vii.

[6] Helpful summaries of recent scholarship in American labor history are Thomas A. Kruger, "American Labor Historiography, Old and New," *Journal of Social History*, 4 (1971): 277–85; Robert H. Zieger, "Workers and Scholars: Recent Trends in American Labor Historiography," *Labor History*, 13 (1972): 245–66; and Paul Faler, "Working Class Historiography," *Radical America*, 3 (1969): 56–68. Innovative works in the field that have broken away from the traditional conceptual framework include especially Richard B. Morris, *Government and Labor in Early America* (New York, 1946); David Brody, *Steelworkers in America: The Non-Union Era* (Cambridge, 1960); Stephan Thernstrom, *Poverty and Progress: Social Mobility in a Nineteenth Century City* (Cambridge, 1964); David Montgomery, *Beyond Equality: Labor and the Radical Republicans, 1862–1872* (New York, 1967); Montgomery, "The Working Class of the Preindustrial American City, 1780–1830," *Labor History*, 9 (1968): 1–22; Montgomery, "The Shuttle and the Cross: Weavers and Artisans in the Kensington Riots of 1844," *Journal of Social History*, 5 (1972): 411–46; Alfred F. Young, "The Mechanics and the Jeffersonians: New York, 1789–1801," *Labor History*, 5 (1964): 247–76; and Alexander Saxton, *The Indispensable Enemy: Labor and the Anti-Chinese Movement in California* (Berkeley, 1971).

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framework.[7] Commons and his colleagues asked large questions, gathered important source materials, and put forth impressive ideas. Together with able disciples, they studied the development of the trade union as an institution and explained its place in a changing labor market. But they gave attention primarily to those few workers who belonged to trade unions and neglected much else of importance about the American working population. Two flaws especially marred this older labor history. Because so few workers belonged to permanent trade unions before 1940, its overall conceptualization excluded most working people from detailed and serious study. More than this, its methods encouraged labor historians to spin a cocoon around American workers, isolating them from their own particular subcultures and from the larger national culture. An increasingly narrow "economic" analysis caused the study of American working-class history to grow more constricted and become more detached from larger developments in American social and cultural history and from the writing of American social and cultural history itself. After 1945 American working-class history remained imprisoned by self-imposed limitations and therefore fell far behind the more imaginative and innovative British and Continental European work in the field. In Great Britain, for example, the guideposts fixed by Sidney and Beatrice Webb have been shattered by labor and social historians such as Asa Briggs, Eric Hobsbawm, Henry Pelling, Sidney Pollard, George Rudé, E. P. Thompson, and Brian, J. F. C., and Royden Harrison, among other scholars who have posed new questions, used new methods, and dug deeply into largely neglected primary materials.[8] As a consequence, a rich and subtle new history of the British common people is now being written. Much of value remains to be learned from the older American labor historians, but the time has long been overdue for a critical re-examination of their

[7] The best example of orthodox "Marxist" labor history is Philip S. Foner, *History of the Labor Movement in the United States*, (New York, 1947–65). Emphasis in so-called New Left history on the relationship between "corporate liberalism" and American labor is found in James Weinstein, *Corporate Ideal in the Liberal State, 1900–1918* (Boston, 1968), and in Ronald Radosh, *American Labor and United States Foreign Policy* (New York, 1969). A different approach is found in Jesse Lemisch, "Jack Tar in the Streets: Merchant Seamen in the Politics of the American Revolution," *William and Mary Quarterly*, 25 (1968): 371–407.

[8] This essay draws especially on the methods of analysis in the following works: E. P. Thompson, *Making of the English Working Class* (London, 1963); Thompson, "Time, Work-Discipline, and Industrial Capitalism," *Past and Present*, 38 (1967): 56–97; Thompson, "The Moral Economy of the English Crowd in the Eighteenth Century," *Past and Present*, 50 (1971): 76–136; Sidney Pollard, *Genesis of Modern Management* (Cambridge, 1965); Pollard, "Factory Discipline in the Industrial Revolution," *Economic History Review*, 16 (1963): 254–71; Eric Hobsbawm, *Primitive Rebels and Social Bandits* (Manchester, 1959); Hobsbawm, *Labouring Men* (London, 1964) and especially the essay on "Custom Wages and Workload," 344–70; George Rudé, *Crowd in History* (New York, 1964); George Rudé and Eric Hobsbawm, *Captain Swing* (New York, 1968); Brian Harrison, "Religion and Recreation in Nineteenth Century England," *Past and Present*, 38 (1967): 98–125; Harrison, *Drink and the Victorians* (Pittsburgh, 1971); Asa Briggs, ed., *Chartist Studies* (New York, 1954); Royden Harrison, *Before the Socialists* (London, 1965); J. F. C. Harrison, *The Quest for the New Moral World: Robert Owen and the Owenites in Britain and America* (New York, 1969).

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framework and their methodology and for applying in special ways to the particularities of the American working-class experience the conceptual and methodological break-throughs of our colleagues across the ocean.

The pages that follow give little attention to the subject matter usually considered the proper sphere of labor history (trade-union development and behavior, strikes and lockouts, and radical movements) and instead emphasize the frequent tension between different groups of men and women new to the machine and a changing American society. Not all periods of time are covered: nothing is said of the half century since the First World War when large numbers of Spanish-speaking and rural Southern white and black workers first encountered the factory and the machine.[9] Much recent evidence describing contemporary dissatisfactions with factory work is not examined.[10] Neither are bound workers (factory slaves in the Old South) or nonwhite free laborers, mostly blacks and Asian immigrants and their descendants, given notice. These groups, too, were affected by the tensions that will be described here, a fact that emphasizes the central place they deserve in any comprehensive study of American work habits and changing American working-class behavior.

Nevertheless the focus in these pages is on free white labor in quite different time periods: 1815–43, 1843–93, 1893–1919. The precise years serve only as guideposts to mark the fact that American society differed

[9] The best recent work is Robert Coles, *South Goes North* (Boston, 1972).

[10] The publication in late 1972 of "Work in America" by the Upjohn Institute for Employment Research, a study financed by the U.S. Department of Health, Education, and Welfare, revealed widespread dissatisfactions with work among contemporary blue- and white-collar workers and even their supervisors. The dispute over this finding in government circles is described in *Newsweek*, Jan. 1, 1973, pp. 47–48, and Howard Muson, "The Ranks of the Discontent," New York *Times*, Dec. 31, 1972. Other evidence of dissatisfaction among factory workers with work routines is reported in the New York *Times* Jan. 23, Apr. 2, and Sept. 3, 1972. The April dispatch reported that a University of Michigan survey team described twenty-five aspects of their jobs to factory workers and then asked the workers to rank them in order of importance. Interesting work ranked first; pay was listed second. Absenteeism, the three large Detroit automobile manufacturers reported, had doubled between 1965 and 1972, "increasing from two to three percent . . . to 5 to 6 percent." In some plants, up to fifteen per cent of the workers were absent "on Fridays and Mondays." Quite interesting discussions of contemporary work dissatisfactions are found in Bennett Kremen, "No Pride in This Dust. Young Workers in the Steel Mills," *Dissent* (Winter 1972), 21–28, and Steve Kline, "Henry and His Magic Kabonk Machine," *Boston Globe Magazine*, July 16, 1972, pp. 8–10, 20–24. See also Rochester *Times-Union* (N.Y.), Nov. 29, 1971, for a discussion of obstinate work and leisure habits among Southern white workers fresh to Northern-owned factories. And a brief feature story in the Rochester *Democrat and Chronicle* (N.Y.), Apr. 30, 1972, told about an artisan Santo Badagliacca who seemed to belong to another era. He had moved to Rochester from Sicily in 1956 with his wife and five-year-old daughter. He was then forty and worked for nearly twelve years as a "tailor" for the National Clothing Company, Timely Clothes, and Bond Clothes, Inc. He quit the clothing factories in 1968 and opened a small custom tailoring shop in his home. In four years, not a single order came for a custom-made suit. Three or four persons visited his place weekly but only to have alterations made. Badagliacca explained his decision to quit the factory: "Each day, it's just collars, collars, collars. I didn't work forty years as a tailor just to do that." See also Richard Sennett and Jonathan Cobb, *The Hidden Injuries of Class* (New York, 1972), and William Serrin, *The Company and the Union: The 'Civilized Relationship' of the General Motors Corporation and the United Auto Workers* (New York, 1973).

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Fig. 1. Tagging immigrants in railroad waiting room. Ellis Island, 1926. This family's tags, marked "P.R.R." and "L.V.R.R.," for Pennsylvania Railroad and Lehigh Valley Railroad, suggest they are headed for the anthracite coal region of Pennsylvania. Photograph by Lewis W. Hine. (A fuller collection of Hine's work together with a critical biography and analysis of his place as an artist can be found in Judith Mara Gutman, *The Eyes of Lewis Hine* [scheduled for publication in the fall of 1973] and *Lewis W. Hine and the American Social Conscience* [New York, 1967].) Photograph courtesy George Eastman House Collection.



Fig. 2. Jewish immigrant. Photograph by Lewis W. Hine.
Courtesy George Eastman House Collection.

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Fig. 3. Italian immigrants. Ellis Island, 1905. Photograph by Lewis W. Hine. Courtesy George Eastman House Collection.

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greatly in each period. Between 1815 and 1843 the United States remained a predominantly preindustrial society and most workers drawn to its few factories were the products of rural and village preindustrial culture. Preindustrial American society was not premodern in the same way that European peasant societies were, but it was, nevertheless, premodern. In the half century after 1843 industrial development radically transformed the earlier American social structure, and during this Middle Period (an era not framed around the coming and the aftermath of the Civil War) a profound tension existed between the older American preindustiral social structure and the modernizing institutions that accompanied the development of industrial capitalism. After 1893 the United States ranked as a mature industrial society. In each of these distinctive stages of change in American society, a recurrent tension also existed between native and immigrant men and women fresh to the factory and the demands imposed upon them by the regularities and disciplines of factory labor. That state of tension was regularly revitalized by the migration of diverse premodern native and foreign peoples into an industrializing or a fully industrialized society. The British economic historian Sidney Pollard has described well this process whereby "a society of peasants, craftsmen, and versatile labourers became a society of modern industrial workers." "There was more to overcome," Pollard writes of industrializing England,

> than the change of employment or the new rhythm of work: there was a whole new culture to be absorbed and an old one to be traduced and spurned, there were new surroundings, often in a different part of the country, new relations with employers, and new uncertainties of livelihood, new friends and neighbors, new marriage patterns and behavior patterns of children within the family and without.[11]

That same process occurred in the United States. Just as in all modernizing countries, the United States faced the difficult task of industrializing whole cultures, but in this country the process was regularly repeated, each stage of American economic growth and development involving different first-generation factory workers. The social transformation Pollard described occurred in England between 1770 and 1850, and in those decades premodern British cultures and the modernizing institutions associated primarily with factory and machine labor collided and interacted. A painful transition occurred, dominated the ethos of an entire era, and then faded in relative importance. After 1850 and until quite recently, the British

[11] Pollard, "The Adaptation of the Labour Force," in *Genesis of Modern Management*, 160–208. Striking evidence of the preindustrial character of most American manufacturing enterprises before 1840 is found in Allen Pred, "Manufacturing in the American Mercantile City, 1800–1840," *Annals of the American Association of Geographers*, 56 (1966): 307–25. See also Richard D. Brown, "Modernization and Modern Personality in Early America, 1600–1865: A Sketch of a Synthesis," *Journal of Interdisciplinary History*, 2 (1972): 201–28.

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working class reproduced itself and retained a relative national homogeneity. New tensions emerged but not those of a society continually busy (and worried about) industrializing persons born out of that society and often alien in birth and color and in work habits, customary values, and behavior. "Traditional social habits and customs," J.F.C. Harrison reminds us, "seldom fitted into the patterns of industrial life, and they had . . . to be discredited as hindrances to progress." That happened regularly in the United States after 1815 as the nation absorbed and worked to transform new groups of preindustrial peoples, native whites among them. The result, however, was neither a static tension nor the mere recurrence of similar cycles, because American society itself changed as did the composition of its laboring population. But the source of the tension remained the same, and conflict often resulted. It was neither the conflict emphasized by the older Progressive historians (agrarianism versus capitalism, or sectional disagreement) nor that emphasized by recent critics of that early twentieth-century synthesis (conflict between competing elites). It resulted instead from the fact that the American working class was continually altered in its composition by infusions, from within and without the nation, of peasants, farmers, skilled artisans, and casual day laborers who brought into industrial society ways of work and other habits and values not associated with industrial necessities and the industrial ethos. Some shed these older ways to conform to new imperatives. Others fell victim or fled, moving from place to place. Some sought to extend and adapt older patterns of work and life to a new society. Others challenged the social system through varieties of collective associations. But for all—at different historical moments—the transition to industrial society, as E. P. Thompson has written, "entailed a severe restructuring of working habits—new disciplines, new incentives, and a new human nature upon which these incentives could bite effectively."[12]

Much in the following pages depends upon a particular definition of culture and an analytic distinction between culture and society. Both deserve brief comment. "Culture" as used here has little to do with Oscar Lewis's inadequate "culture of poverty" construct and has even less to do with the currently fashionable but nevertheless quite crude behavioral social history that defines class by mere occupation and culture as some kind of a magical mix between ethnic and religious affiliations.[13] Instead

[12] J. F. C. Harrison, *Learning and Living* (London, 1961), 268; Thompson, "Time, Work-Discipline, and Industrial Capitalism," 57.

[13] Valuable and convincing theoretical criticisms of the culture of poverty construct appear in detail in Eleanor Burke Leacock, ed., *The Culture of Poverty: A Critique* (New York, 1971). See also William Preston's withering comments on the faulty application of the culture of poverty to a recent study of the Industrial Workers of the World: William Preston, "Shall This Be All? U. S. Historians versus William D. Haywood *et al.*," *Labor History*, 12 (1971): 435–71. The use of crude definitions of class and culture in otherwise sophisticated behavioral social history is as severely criticized in James Green, "Behavioralism and Class Analysis," *Labor History*, 13 (1972): 89–106.

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this paper has profited from the analytic distinctions between culture and society made by the anthropologists Eric Wolf and Sidney W. Mintz and the exiled Polish sociologist Zygmunt Bauman. Mintz finds in culture "a kind of resource" and in society "a kind of arena," the distinction being "between sets of historically available alternatives or forms on the one hand, and the societal circumstances or settings within which these forms may be employed on the other." "Culture," he writes, "is *used*; and any analysis of its use immediately brings into view the arrangements of persons in societal groups for whom cultural forms confirm, reinforce, maintain, change, or deny particular arrangements of status, power, and identity." Bauman insists that for analytic purposes the two (culture and society) need always be examined discretely to explain behavior:

> Human behavior, whether individual or collective, is invariably the resultant of two factors: the cognitive system as well as the goals and patterns of behavior as defined by culture systems, on the one hand, and the system of real contingencies as defined by the social structure on the other. A complete interpretation and apprehension of social processes can be achieved only when both systems, as well as their interaction, are taken into consideration.

Such an analytic framework allows social historians to avoid the many pitfalls that follow implicit or explicit acceptance of what the anthropologist Clifford Geertz calls "the theoretical dichotomies of classical sociology—*Gemeinschaft* and *Gesellschaft,* mechanic and organic solidarity, [and] folk and urban cultures." Too often, the subtle historical processes that explain particular patterns of working-class and other behavior have been viewed as no more than "the expansion of one at the expense of the other."[14] An analytic model that distinguishes between culture and society reveals that even

[14] Eric Wolf, "Specific Aspects of Plantation Systems in the New World: Community Sub-Cultures and Social Class," in *Plantation Systems of the New World* (Washington, 1949), 142; Sidney W. Mintz, "Foreword," in Norman Whitten and John F. Szwed, eds., *Afro-American Anthropology: Contemporary Perspectives* (New York, 1970), 1–16 but especially 9–10; Zygmunt Bauman, "Marxism and the Contemporary Theory of Culture," *Co-Existence,* 5 (1968): 171–98; Clifford Geertz, *Old Societies and New States* (Glencoe, 1963), 32–54, 109–10, 154–55. See also Emilio Willems, "Peasantry and City: Cultural Persistence and Change in Historical Perspective, A European Case," *American Anthropologist,* 72 (1970): 528–43, in which Willems disputes the proposition that "peasant culture is incompatible with industrialization" and shows that in the German Rhineland town of Neyl there existed significant "cultural continuity of urban lower class and peasantry rather than cultural polarity between the two segments." A brilliant article which focuses on West Indian slaves but is nevertheless methodologically useful to students of all lower-class cultures is S. W. Mintz, "Toward an Afro-American History," *Journal of World History,* 13 (1971): 317–33. The confusion between race and culture greatly marred early twentieth-century American labor history, and no one revealed that more clearly than John R. Commons in *Races and Immigrants in America* (New York, 1907), 7, 11–12, 153–54, 173–75, *passim.* "Race differences," Commons believed, "are established in the very blood and physical condition" and "most difficult to eradicate." Changes might take place in language and other behavioral patterns, "but underneath all these changes there may continue the physical, mental, and moral incapacities which determine the real character of their religion, government, industry, and literature." The behavior of the recent immigrants confused historians like Commons. His racial beliefs and the crude environmentalism he shared with other Progressive reformers encouraged that confusion. "Ireland and Italy," he could write, "have nothing to compare to the trade-union movement

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in periods of radical economic and social change powerful cultural continuities and adaptations continued to shape the historical behavior of diverse working-class populations. That perspective is especially important in examining the premodern work habits of diverse American men and women and the cultural sanctions sustaining them in an alien society in which the factory and the machine grew more and more important.

Men and women who sell their labor to an employer bring more to a new or changing work situation than their physical presence. What they bring to a factory depends, in good part, on their culture of origin, and how they behave is shaped by the interaction between that culture and the particular society into which they enter. Because so little is yet known about preindustrial American culture and subcultures, some caution is necessary in moving from the level of generalization to historical actuality. What follows compares and contrasts working people new to industrial society but living in quite different time periods. First, the expectations and work habits of first-generation predominantly native American factory workers before 1843 are compared with first-generation immigrant factory workers between 1893 and 1920. Similarities in the work habits and expectations of men and women who experienced quite different premodern cultures are indicated. Second, the work habits and culture of artisans in the industrializing decades (1843–93) are examined to indicate the persistence of powerful cultural continuities in that era of radical economic change. Third, evidence of premodern working-class behavior that parallels European patterns of premodern working-class behavior in the early phases of industrialization is briefly described to suggest that throughout the entire period (1815–1920) the changing composition of the American working class caused the recurrence of "premodern" patterns of collective behavior usually only associated with the early phases of industrialization. And, finally, attention is given to some of the larger implications resulting from this recurrent tension between work, culture, and society.

THE WORK HABITS and the aspirations and expectations of men and women new to factory life and labor are examined first. Common work habits rooted in diverse premodern cultures (different in many ways but never-

of England, but the Irish are the most effective organizers of the American unions, and the Italians are becoming the most ardent unionists. Most remarkable of all, the individualistic Jew from Russia, contrary to his race instinct, is joining the unions." "The American unions, in fact," Commons concluded, "grow out of American conditions, and are an American product." But he could not explain how these "races" so easily adapted to American conditions. How could he when he believed that "even the long series of crimes against the Indians, to which the term 'Century of Dishonor' seems to have attached itself with no protest, must be looked upon as a mob spirit of a superior race bent on despoiling a despised and inferior race"?

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theless all ill fitted to the regular routines demanded by machine-centered factory processes) existed among distinctive first-generation factory workers all through American history. We focus on two quite different time periods: the years before 1843 when the factory and machine were still new to America and the years between 1893 and 1917 when the country had become the world's industrial colossus. In both periods workers new to factory production brought strange and seemingly useless work habits to the factory gate. The irregular and undisciplined work patterns of factory hands before 1843 frustrated cost-conscious manufacturers and caused frequent complaint among them. Textile factory work rules often were designed to tame such rude customs. A New Hampshire cotton factory that hired mostly women and children forbade "spirituous liquor, smoking, nor any kind of amusement . . . in the workshops, yards, or factories" and promised the "immediate and disgraceful dismissal" of employees found gambling, drinking, or committing "any other debaucheries." A Massachusetts firm nearby insisted that young workers unwilling to attend church stay "within doors and improve their time in reading, writing, and in other valuable and harmless employment." Tardy and absent Philadelphia workers paid fines and could not "carry into the factory nuts, fruits, etc.; books or paper." A Connecticut textile mill owner justified the twelve-hour day and the six-day week because it kept "workmen and children" from "vicious amusements." He forbade "gaming . . . in any private house." Manufacturers elsewhere worried about the example "idle" men set for women and children. Massachusetts family heads who rented "a piece of land on shares" to grow corn and potatoes while their wives and children labored in factories worried one manufacturer. "I would prefer giving constant employment at some sacrifice," he said, "to having a man of the village seen in the streets on a rainy day at leisure." Men who worked in Massachusetts woolen mills upset expected work routines in other ways. "The wool business requires more man labour," said a manufacturer, "and this we study to avoid. Women are much more ready to follow good regulations, are not captious, and do not clan as the men do against the overseers." Male factory workers posed other difficulties, too. In 1817 a shipbuilder in Medford, Massachusetts, refused his men grog privileges. They quit work, but he managed to finish a ship without using further spirits, "a remarkable achievement." An English visitor in 1832 heard an American complain that British workers in the Paterson cotton and machine shops drank excessively and figured as "the most beastly people I have ever seen." Four years later a New Jersey manufacturer of hats and caps boasted in a public card that he finally had "4 and 20 good, permanent workmen," not one infected with "the brutal leprosy of blue Monday habits and the moral gangrene of 'trades union' principles." Other manufacturers had less good fortune. Absenteeism occurred frequently among the Pennsylvania iron workers at

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the rural Hopewell Village forge: hunting, harvesting, wedding parties, frequent "frolicking" that sometimes lasted for days, and uproarious Election and Independence Day celebrations plagued the mill operators. In the early nineteenth century, a New Jersey iron manufacturer filled his diary with notations about irregular work habits: "all hands drunk"; "Jacob Ventling hunting"; "molders all agree to quit work and went to the beach"; "Peter Cox very drunk and gone to bed. Mr. Evans made a solemn resolution any person or persons bringing liquor to the work enough to make drunk shall be liable to a fine"; "Edward Rutter off a-drinking. It was reported he got drunk on cheese."[15]

Employers responded differently to such behavior by first-generation factory hands. "Moral reform" as well as what Sidney Pollard calls carrot-and-stick policies meant to tame or to transform such work habits. Fining was common. Hopewell Furnace managers deducted one dollar from Samuel York's wages "for getting intoxesitated [*sic*] with liquer [*sic*] and neglecting hauling 4 loads wash Dird at Joneses." Special material rewards encouraged steady work. A Hopewell Village blacksmith contracted for nineteen dollars a month, and "if he does his work well we are to give him a pair of coarse boots." In these and later years manufacturers in Fall River and Paterson institutionalized traditional customs and arranged for festivals and parades to celebrate with their workers a new mill, a retiring superintendent, or a finished locomotive. Some rewarded disciplined workers in special ways. When Paterson locomotive workers pressed for higher wages, their employer instructed an underling: "Book keeper, make up a roll of the men . . . making *fulltime*; if they can't support their families on the wages they are now getting, they must have more. But the other men, who are drunk every Monday morning, I don't want them around the shop under any circumstances." Where factory work could be learned easily, new hands replaced irregular old ones. A factory worker in New England remembered that years before the Civil War her employer had hired "all American girls" but later shifted to immigrant laborers because "not coming from country homes, but living as the Irish do, in the town, they take no vacations, and can be relied on at the mill all year round." Not all such devices worked to the satisfaction of workers or their employers. Sometime in the late 1830s

[15] *Mechanic's Free Press* (Philadelphia), Jan. 17, 1829; Edith Abbott, *Women in Industry* (New York, 1910), 374–75; Silesia Factory Rules, Germantown *Telegraph*, Nov. 6, 1833, reprinted in William Sullivan, *Industrial Worker in Pennsylvania* (Harrisburg, 1955), 34; letters of Smith Wilkinson and Jedidiah Tracy to George White, n.d., printed in George White, *Memoir of Samuel Slater* (Philadelphia, 1836), 125–32; Carroll D. Wright, *Industrial Evolution of the United States* (New York, 1901), 296; Rowland T. Berthoff, *British Immigrants in Industrial America* (Cambridge, 1953), 146; Card of H. B. Day, 1836, printed in Paterson *Guardian* (N.J.), Aug. 6, 1886; J. E. Walker, *Hopewell Village* (Philadelphia, 1966), 115–16, 256, 265–68, 282–83, 331, 380–84; "The Martha Furnace Diary," in A. D. Pierce, *Iron in the Pines* (New Brunswick, 1957), 96–105; Sidney Pollard, "Factory Discipline in the Industrial Revolution," *Economic History Review*, 16 (1963): 254–71.

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merchant capitalists sent a skilled British silk weaver to manage a new mill in Nantucket that would employ the wives and children of local whalers and fishermen. Machinery was installed, and in the first days women and children besieged the mill for work. After a month had passed, they started dropping off in small groups. Soon nearly all had returned "to their shore gazing and to their seats by the sea." The Nantucket mill shut down, its hollow frame an empty monument to the unwillingness of resident women and children to conform to the regularities demanded by rising manufacturers.[16]

First-generation factory workers were not unique to premodern America. And the work habits common to such workers plagued American manufacturers in later generations when manufacturers and most native urban whites scarcely remembered that native Americans had once been hesitant first-generation factory workers.[17] To shift forward in time to East and South European immigrants new to steam, machinery, and electricity and new to the United States itself is to find much that seems the same. American society, of course, had changed greatly, but in some ways it is as if a film—run at a much faster speed—is being viewed for the second time: primitive work rules for unskilled labor, fines, gang labor, and subcontracting were commonplace. In 1910 two-thirds of the workers in twenty-one major manufacturing and mining industries came from Eastern and Southern Europe or were native American blacks, and studies of these "new immigrants" record much evidence of preindustrial work habits among the men and women new to American industry. According to Moses Rischin, skilled immigrant Jews carried to New York City town and village employment patterns, such as the *landsmannschaft* economy and a preference for small shops as opposed to larger factories, that sparked frequent disorders but hindered stable trade unions until 1910. Specialization spurred anxiety: in Chicago Jewish glovemakers resisted the subdivision of labor even though it promised better wages. "You shrink from doing either kind of work itself, nine hours a day," said two observers of these immigrant women. "You cling to the variety . . . , the mental luxury of first, finger-sides, and then, five separate leather pieces, for relaxation, to play with! *Here* is a luxury worth fighting for!" American work rules also conflicted with religious imperatives. On the eighth day after the birth of a son, Orthodox Jews in Eastern Europe held a festival, "an occasion of much rejoicing." But the American work

[16] Walker, *Hopewell Village, passim;* Walker, "Labor-Management Relations at Hopewell Village," *Labor History,* 14 (1973): 3–18; *Voice of Industry* (Lowell), Jan. 8, 1847; New York *Tribune,* June 29, July 4, Aug. 20, 1853; Paterson *Guardian,* Sept. 13, 1886; Massachusetts Bureau of Labor Statistics, *First Annual Report, 1869–1870* (Boston, 1870), 119; Paterson *Evening News,* Nov. 21, 1900.

[17] Fining as means of labor discipline, of course, remained common between 1843 and 1893. See, for examples, *Illinois Bureau of Labor Statistics, Fourth Annual Report, 1886* (Springfield, 1887), 501–26; Pennsylvania Bureau of Labor Statistics, *Fourteenth Annual Report, 1886* (Harrisburg, 1887), 13–14.

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week had a different logic, and if the day fell during the week the celebration occurred the following Sunday. "The host . . . and his guests," David Blaustein remarked, "know it is not the right day," and "they fall to mourning over the conditions that will not permit them to observe the old custom." The occasion became "one for secret sadness rather than rejoicing." Radical Yiddish poets, like Morris Rosenfeld, the presser of men's clothing, measured in verse the psychic and social costs exacted by American industrial work rules:

> The Clock in the workshop,—it rests not a moment;
> It points on, and ticks on: eternity—time;
> Once someone told me the clock had a meaning,—
> In pointing and ticking had reason and rhyme. . . .
> At times, when I listen, I hear the clock plainly;—
> The reason of old—the old meaning—is gone!
> The maddening pendulum urges me forward
> To labor and still labor on.
> The tick of the clock is the boss in his anger.
> The face of the clock has the eyes of the foe.
> The clock—I shudder—Dost hear how it draws me?
> It calls me "Machine" and it cries [to] me "Sew"![18]

Slavic and Italian immigrants carried with them to industrial America subcultures quite different from that of village Jews, but their work habits were just as alien to the modern factory. Rudolph Vecoli has reconstructed Chicago's South Italian community to show that adult male seasonal construction gangs as contrasted to factory labor were one of many traditional customs adapted to the new environment, and in her study of South Italian peasant immigrants Phyllis H. Williams found among them men who never adjusted to factory labor. After "years" of "excellent" factory work, some "began . . . to have minor accidents" and others "suddenly give up and are found in their homes complaining of a vague indisposition with no apparent physical basis." Such labor worried early twentieth-century efficiency experts, and so did Slavic festivals, church holidays, and "prolonged merriment." "Man," Adam Smith wisely observed, "is, of all sorts of luggage, the most difficult to be transported." That was just as true for these Slavic immigrants as for the early nineteenth-century native American factory workers. A Polish wedding in a Pennsylvania mining or mill town lasted between three and five days. Greek and Roman Catholics shared the same jobs but had different holy days, "an annoyance to many employers." The Greek Church had "more than eighty festivals in the year," and "the Slav religiously observes the days on which the saints are commemorated and invariably takes a holiday." A celebration of the

[18] Moses Rischin, *Promised City: New York's Jews, 1870–1914* (Cambridge, 1962), 19–33, 144–99 but especially 181–82; New York *Tribune*, Aug. 16, 1903; William Herd and Rheta C. Dorr, "The Women's Invasion," *Everybody's Magazine*, Mar. 1909, pp. 375–76; Melech Epstein, *Jewish Labor in the United States* (New York, 1950), 280–85, 290–91.

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Fig. 4, above. Italian canal construction workers in western New York playing cards in a shack. Photograph by Lewis W. Hine. Courtesy George Eastman House Collection.

Fig. 5, below. Native white textile-mill worker in the South, 1912. Photograph by Lewis W. Hine. Courtesy George Eastman House Collection.

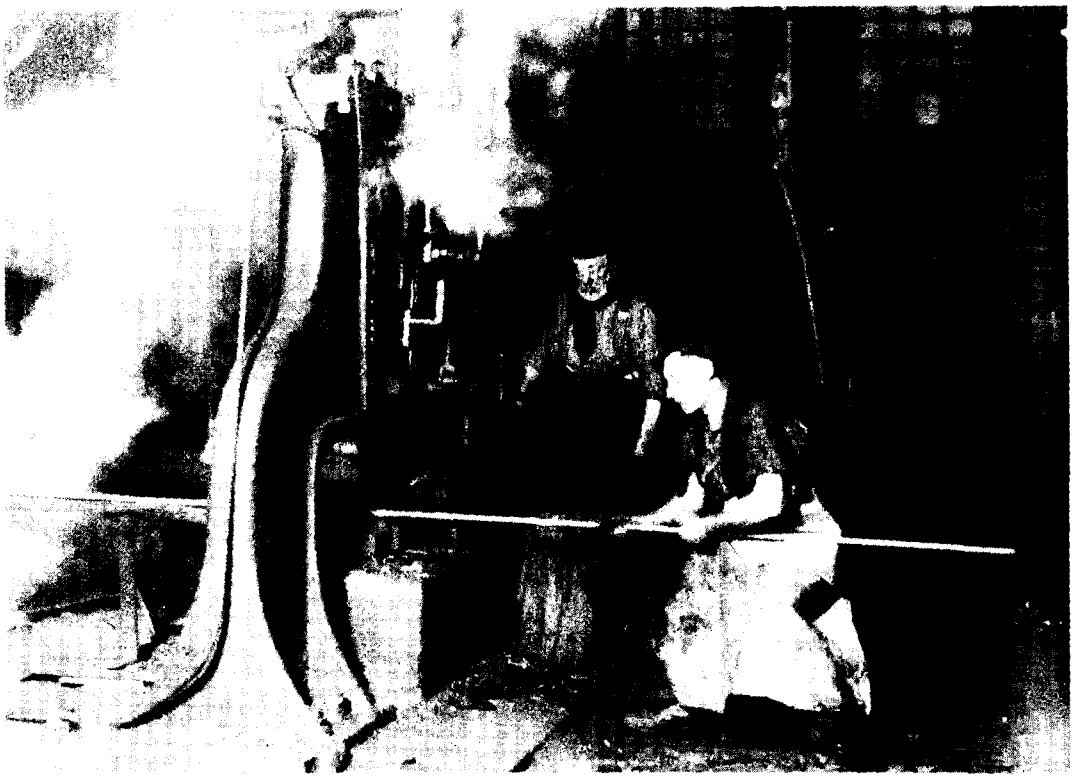


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Fig. 6, above. Italian workers in a New York tenement-house sweatshop, 1909. Photograph by Lewis W. Hine. Courtesy George Eastman House Collection.

Fig. 7, below. Shaping rods under a trip hammer in an iron or steel mill in the Pittsburgh area. Note the absence of machine processes. Photograph by Lewis W. Hine. Courtesy George Eastman House Collection.



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American Day of Independence in Mahanoy City, Pennsylvania, caught the eye of a hostile observer. Men parading the streets drew a handcart with a barrel of lager in it. Over the barrel "stood a comrade, goblet in hand and crowned with a garland of laurel, singing some jargon." Another sat and played an accordion. At intervals, the men stopped to "drink the good beverage they celebrated in song." The witness called the entertainment "an imitation of the honor paid Bacchus which was one of the most joyous festivals of ancient Rome" and felt it proof of "a lower type of civilization." Great Lakes dock workers "believed that a vessel could not be unloaded unless they had from four to five kegs of beer." (And in the early irregular strikes among male Jewish garment workers, employers negotiated with them out of doors and after each settlement "would roll out a keg of beer for their entertainment of the workers.") Contemporary betters could not comprehend such behavior. Worried over a three-day Slavic wedding frolic, a woman concluded: "You don't think they have souls, do you? No, they are beasts, and in their lust they'll perish." Another disturbed observer called drink "un-American, . . . a curse worse than the white plague." About that time, a young Italian boy lay ill in a hospital. The only English words he knew were "boots" and "hurry up."[19]

More than irregular work habits bound together the behavior of first-generation factory workers separated from one another by time and by the larger structure of the society they first encountered. Few distinctive American working-class populations differed in so many essentials (their sex, their religions, their nativity, and their prior rural and village cultures) as the Lowell mill girls and women of the Era of Good Feelings and the South and East European steel workers of the Progressive Era. To describe similarities in their expectations of factory labor is not to blur these important differences but to suggest that otherwise quite distinctive men and women interpreted such work in similar ways. The Boston Associates, pioneer American industrialists, had built up Lowell and other towns like it to overcome early nineteenth-century rural and village prejudices and fears about factory work and life and in their regulation of working-class social habits hoped to assure a steady flow of young rural women ("girls") to and from the looms. "The sagacity of self-interest as well as more disinterested considerations," explained a Lowell clergyman in 1845, "has led to the adoption of a strict system of moral police." Without "sober, orderly, and moral" workers, profits would be "absorbed by cases of irregularity, carelessness, and neglect." The Lowell capitalists

[19] William M. Leiserson, *Adjusting Immigrant and Industry* (New York, 1924), ch. 1; R. J. Vecoli, "Contadini in Chicago: A Critique of 'The Uprooted'," *Journal of American History*, 51 (1964): 404–27; Phyllis H. Williams, *South Italian Folkways in Europe and America* (New Haven, 1938), 30–32; A. Rosenberg, *Memoirs of a Cloak Maker* (New York, 1920), 42, quoted in Louis Levine, *Women's Garment Workers* (New York, 1924), 42; Peter Roberts, *New Immigration* (New York, 1912), 79–97, 118–19; Roberts, *Anthracite Communities* (New York, 1904), 49–56, 219, 236, 291, 294–95.

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thrived by hiring rural women who supplemented a distant family's income, keeping them a few years, and then renewing the process. Such steady labor turnover kept the country from developing a permanent proletariat and so was meant to assure stability. Lowell's busy cotton mills, well-ordered boarding houses, temples of religion and culture, factory girls, and moral police so impressed Anthony Trollope that he called the entire enterprise a "philanthropic manufacturing college." John Quincy Adams thought the New England cotton mills "palaces of the Poor," and Henry Clay marveled over places like the Lowell mills. "Who has not been delighted with the clock-work movements of a large cotton factory?" asked the father of the American System. The French traveler Michel Chevalier had a less sanguine reaction. He found Lowell "neat and decent, peaceable and sage," but worried, "Will this become like Lancashire? Does this brilliant glare hide the misery and suffering of the working girls?"[20]

Historians of the Lowell mill girls find little evidence before 1840 of organized protest among them and attribute their collective passivity to corporation policing policies, the frequent turnover in the labor force, the irregular pace of work (after it was rationalized in the 1840s, it provoked collective protest), the freedom the mill girls enjoyed away from rural family dominance, and their relatively decent earnings. The women managed the transition to mill life because they did not expect to remain factory workers too long. Nevertheless frequent inner tension revealed itself among the mobile mill women. In an early year, a single mill discharged twenty-eight women for such reasons as "misconduct," "captiousness," "disobedience," "impudence," "levity," and even "mutiny." The difficult transition from rural life to factory work also caused tensions outside the mills. Rural girls and women, Harriet Robinson later recalled, came to Lowell in "outlandish fashions" and with "queer names," "Samantha, Triphena, Plumy, Kezia, Aseneth, Elgardy, Leafy, Ruhamah, Almaretta, Sarpeta, and Florilla . . . among them." They spoke a "very peculiar" dialect ("a language almost unintelligible"). "On the broken English and Scotch of their ancestors," said Robinson, "was engrafted the nasal Yankee twang." Some soon learned the "city way of speaking"; others changed their names to "Susan" or "Jane"; and for still others new clothing, especially straw hats, became important. But the machines they worked still left them depressed and with feelings of anxiety. "I never cared much for machinery," Lucy Larcom said of her early Lowell years. "I could not see into their complications or feel interested in them. . . . In sweet June weather I would lean far out of the window, and

[20] Anthony Trollope, quoted in Howard Gitelman, "The Waltham System and the Coming of the Irish," *Labor History*, 8 (1967): 227–54; John Quincy Adams and Henry Clay quoted in Seth Luther, *An Address to the Workingmen of New England* (Boston, 1832), title page; Michel Chevalier, *Society, Manners, and Politics in the United States* (Boston, 1939; reprinted New York, 1969), 133–44; Henry Miles, *Lowell As It Is and Was* (Lowell, 1845), 128–46.

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try not to hear the unceasing clash of sound inside." She kept a plant beside her and recollected an overseer who confiscated newspaper clippings and even the pages of a "torn Testament" some women had slipped into the factory. Years after she had left the textile mills, Lucy Larcom ridiculed her mill-girl poems: "I continued to dismalize myself at times quite unnecessarily." Their titles included "The Early Doomed" and "The Complaint of a Nobody" (in which she compared herself to "a weed growing up in a garden"). When she finally quit the mill, the paymaster asked, "Going where you can earn more money?" "No," she remembered answering, "I am going where I can have more time." "Ah, yes!" he responded, "time is money."[21]

Even the *Lowell Offering* testified to the tensions between mill routines and rural rhythms and feelings. Historians have dismissed it too handily because the company sponsored it and refused to publish prose openly critical of mill policies. But the fiction and poetry of its contributors, derivative in style and frequently escapist, also often revealed dissatisfactions with the pace of work. Susan, explaining her first day in the mill to Ann, said the girls awoke early and one sang, "Morning bells, I hate to hear./Ringing dolefully, loud and clear." Susan went on:

> You cannot think how odd everything seemed to me. I wanted to laugh at everything, but did not know what to make sport of first. They set me to threading shuttles, and tying weaver's knots and such things, and now I have improved so that I can take care of one loom. I could take care of two if I only had eyes in the back of my head. . . . When I went out at night, the sound of the mill was in my ears, as of crickets, frogs, and Jew-harps, all mingled together in strange discord. After, it seemed as though cotton-wool was in my ears. But now I do not mind it at all. You know that people learn to sleep with the thunder of Niagara in their ears, and the cotton mill is no worse.

Ellen Collins quit the mill, complaining about her "obedience to the ding-dong of the bell—just as though we were so many living machines." In "A Weaver's Reverie," Ella explained why the mill women wrote "so much about the beauties of nature."

> Why is it that the delirious dreams of the famine-stricken are of tables loaded with the richest viands? . . . Oh, tell me why this is, and I will tell you why the factory girl sits in the hours of meditation and thinks, not of the crowded, clattering mill, nor of the noisy tenement which is her home.

Contemporary labor critics who scorned the *Lowell Offering* as little more than the work of "poor, caged birds," who "while singing of the roses . . . forget the bars of their prison," had not read it carefully. Their

[21] Roll Book of the Hamilton Company, 1826–27, printed in Carolina Ware, *Early New England Cotton Manufacture* (Boston, 1924), 266–67; Harriet Robinson, *Loom and Spindle* (New York, 1898), 62–69; Lucy Larcom, *A New England Girlhood* (Boston, 1889), 138–43, 152–55, 174–76, 180–85, 209–19, 226–31.

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attachment to nature was the concern of persons working machines in a society still predominantly "a garden," and it was not unique to these Lowell women. In New Hampshire five hundred men and women petitioned the Amoskeag Manufacturing Company's proprietors in 1853 not to cut down an elm tree to allow room for an additional mill: "It was a beautiful and goodly tree" and belonged to a time "when the yell of the red man and the scream of the eagle were alone heard on the banks of the Merrimack, instead of two giant edifices filled with the buzz of busy and well-remunerated industry." Each day, the workers said, they viewed that tree as "a connecting link between the past and the present," and "each autumn [it] remind[s] us of our own mortality."[22]

Aspirations and expectations interpret experience and thereby help shape behavior. Some Lowell mill girls revealed dissatisfactions, and others made a difficult transition from rural New England to that model factory town, but that so few planned to remain mill workers eased that transition and hampered collective protest. Men as well as women who expect to spend only a few years as factory workers have little incentive to join unions. That was just as true of the immigrant male common laborers in the steel mills of the late nineteenth and early twentieth centuries (when multi-plant oligopoly characterized the nation's most important manufacturing industry) as in the Lowell cotton mills nearly a century earlier. David Brody has explained much about the common laborers. In those years, the steel companies successfully divorced wages from productivity to allow the market to shape them. Between 1890 and 1910, efficiencies in plant organization cut labor costs by about a third. The great Carnegie Pittsburgh plants employed 14,359 common laborers, 11,694 of them South and East Europeans. Most, peasant in origin, earned less than $12.50 a week (a family needed fifteen dollars for subsistence). A staggering accident rate damaged these and other men: nearly twenty-five per cent of the recent immigrants employed at the Carnegie South Works were injured or killed each year between 1907 and 1910, 3,723 in all. But like the Lowell mill women, these men rarely protested in collective ways, and for good reason. They did not plan to stay in the steel mills long. Most had come to the United States as single men (or married men who had left their families behind) to work briefly in the mills, save some money, return home, and purchase farm land. Their private letters to European relatives indicated a realistic awareness of

[22] William Scoresby, *American Factories and Their Mill Operatives* (Boston, 1845), 21–23, 58–66, *passim;* Norman Ware, *Industrial Worker, 1840–1860* (New York, 1924), 85; "New York Industrial Exhibition," *Sessional Papers* (Commons) 1854, vol. 26, p. 10; Ray Ginger, "Labor in a Massachusetts Cotton Mill," *Business History Review,* 28 (1954): 67–91 (a brilliant study of mobility among New England factory women). Useful works on the early New England cotton mills and their female workers include Ware, *Early New England Cotton Manufacture;* Hannah Josephson, *Golden Threads, Mill Girls and Magnates* (New York, 1949); Vera Shlakman, "Economic History of a Factory Town: A Study of Chicopee, Massachusetts," *Smith College Studies in History,* 20, nos. 1–4 (1934–35); Edith Abbott, *Women in Industry.*

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their working life that paralleled some of the Lowell fiction: "if I don't earn $1.50 a day, it would not be worth thinking about America"; "a golden land so long as there is work"; "here in America one must work for three horses"; "let him not risk coming, for he is too young"; "too weak for America." Men who wrote such letters and avoided injury often saved small amounts of money, and a significant number fulfilled their expectations and quit the factory and even the country. Forty-four South and East Europeans left the United States for every one hundred that arrived between 1908 and 1910. Not a steel worker, a young Italian boy living in Rochester, New York, summed up the expectations of many such immigrant men in a poem he wrote after studying English just three months:

> Nothing job, nothing job,
> I come back to Italy;
> Nothing job, nothing job,
> Adieu, land northerly. . . .
>
> Nothing job, nothing job,
> O! sweet sky of my Italy;
> Nothing job, nothing job,
> How cold in this country. . . .
>
> Nothing job, nothing job,
> I return to Italy;
> Comrades, laborers, good-bye;
> Adieu, land of "Fourth of July."[23]

Immigrant expectations coincided for a time with the fiscal needs of industrial manufacturers. The Pittsburgh steel magnates had as much good fortune as the Boston Associates. But the stability and passivity they counted on among their unskilled workers depended upon steady work and the opportunity to escape the mills. When frequent recessions caused recurrent unemployment, immigrant expectations and behavior changed. What Brody calls peasant "group consciousness" and "communal loyalty" sustained bitter wildcat strikes after employment picked up. The tenacity of these immigrant strikes for higher wages amazed contemporaries, and brutal suppression often accompanied them (Cleveland, 1899; East Chicago, 1905; McKees Rock, 1909; Bethlehem, 1910; and Youngstown in 1915 where, after a policeman shot into a peaceful parade, a riot caused an estimated one million dollars in damages). The First World War and its aftermath blocked the traditional route of overseas outward mobility, and the consciousness of immigrant steel workers changed. They sparked the 1919 steel strike. The steel mill had become a way of life for

[23] David Brody, *Steelworkers in America: The Non-Union Era* (Cambridge, 1960), 26–28, 36, 96–111, 119–20, 125–46, 180–86, *passim:* Brody, *Labor in Crisis* (Philadelphia, 1965), 15–45; "Song of an Italian Workman," Rochester *Post-Express* (N.Y.), n.d., reprinted in *Survey,* 21 (1908): 492–93.

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them and was no longer the means by which to reaffirm and even strengthen older peasant and village life-styles.[24]

LET US SHARPLY shift the time perspective from the years before 1843 and those between 1893 and 1919 to the decades between 1843 and 1893 and also shift our attention to the artisans and skilled workers who differed so greatly in the culture and work-styles they brought to the factory from men and women bred in rural and village cultures. The focus, however, remains the same—the relationship between settled work habits and culture. This half century saw the United States (not small pockets within it) industrialize as steam and machinery radically transformed the premodern American economic structure. That so much attention has been given to the Civil War as a crucial divide in the nation's history (and it was, of course, for certain purposes) too frequently has meant neglect by historians of common patterns of behavior that give coherence to this period. Few contemporaries described these large structural changes more effectively if indirectly than the Boston labor reformer Jennie Collins in 1871:

> If you should enter a factory and find the water-wheels in the garret, the heaviest machinery in the seventh story, and the dressing and weaving in the basement, you would find the machinery and system less out of joint than at present it seems to be in this strange country of ours. The structure of our society is like a building for which the stones were carefully designed and carved, but in the construction of which the masons seized upon whatever block came handiest, without regard to design or fitness, using window-sills for partition walls, capstones for the foundation, and chink-pieces for the corner-stone.

The magnitude of the changes noticed by Collins cannot be understated. In 1869 half of the country's manufacturing enterprises still managed on water power. The nation in 1860 counted more slaves than factory workers. In his unpublished study of six upstate New York counties Richard L. Ehrlich has found that in five counties during that same year employment in manufacturing plants having at least fifty workers accounted for thirty-seven per cent or less of their respective labor forces. In the six counties (Albany, Erie, Monroe, Oneida, Onondaga, and Rensselaer) the average number of persons employed by firms engaging fewer than fifty employees was less than nine, In the year of Abraham Lincoln's election as president, the United States ranked behind England, France, and Germany in the value of its manufactured product. In 1894 the United States led the field: its manufactured product nearly equalled in value that of Great Britain, France, and Germany together. But such profound economic changes did not entirely shatter the older American social structure and the settled cultures of premodern native and immigrant American artisans.

[24] Brody, *Steelworkers in America, passim;* Brody, *Labor in Crisis,* 15–45.

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"There is no such thing as economic growth which is not, at the same time, growth or change of a culture," E. P. Thompson has written. Yet he also warns that "we should not assume any automatic, or over-direct, correspondence between the dynamic of economic growth and the dynamic of social or cultural life." That significant stricture applies as much to the United States as to England during its Industrial Revolution and especially to native and immigrant artisans between 1843 and 1893.[25]

It is not surprising to find tenacious artisan work habits before the Civil War, what Thompson calls "alternate bouts of intense labour and of idleness wherever men were in control of their working lives." An English cabinetmaker shared a New York City workplace with seven others (two native Americans, two Germans, and one man each from Ireland, England, and France), and the readers of *Knight's Penny Magazine* learned from him that "frequently . . . after several weeks of real hard work . . . a simultaneous cessation from work took place." "As if . . . by tacit agreement, every hand" contributed "loose change," and an apprentice left the place and "speedily returned laden with wine, brandy, biscuits, and cheese." Songs came forth "from those who felt musical," and the same near-ritual repeated itself two more times that day. Similar relaxations, apparently self-imposed, also broke up the artisans' work day in the New York City shipyards, and a ship carpenter described them as "an indulgence that custom had made as much of a necessity in a New York shipyard as a grind-stone":

> In our yard, at half-past eight a.m., Aunt Arlie McVane, a clever kind-hearted, but awfully uncouth, rough sample of the "Ould Sod," would make her welcome appearance in the yard with her two great baskets, stowed and checked off with crullers, doughnuts, ginger-bread, turnovers, pieces, and a variety of sweet cookies and cakes; and from the time Aunt Arlie's baskets came in sight until every man and boy, bosses and all, in the yard, had been supplied, always at one cent a piece for any article on the cargo, the pie, cake and cookie trade was a brisk one. Aunt Arlie would usually make the rounds of the yard and supply all hands in about an hour, bringing the forenoon up to half-past nine, and giving us from ten to fifteen minutes "breathing spell" during lunch; no one ever hurried during "cake-time."

Nor was this all:

> After this was over we would fall to again, until interrupted by Johnnie Gogean, the English candyman, who came in always at half-past ten, with his great board, the size of a medium extension dining table, slung before him, covered with all sorts of "stick", and several of sticky candy, in one-cent lots. Bosses, boys and men —all hands, everybody—invested one to three cents in Johnnie's sweet wares,

[25] Jennie Collins, *Nature's Aristocracy* (Boston, 1871), 4; Richard L. Ehrlich, "The Development of Manufacturing in Selected Counties in the Erie Canal Corridor, 1815–1860," (Ph.D. dissertation, State University of New York, Buffalo, 1972); Stuart Bruchey, *Roots of American Economic Growth* (New York, 1965), 139; George Rogers Taylor, *Transportation Revolution, 1815–1860* (New York, 1951), 249; Thompson, *Making of the English Working Class*, 97, 192.

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and another ten to fifteen minutes is spent in consuming it. Johnnie usually sailed out with a bare board until 11 o'clock at which time there was a general sailing out of the yard and into convenient grog-shops after whiskey; only we had four or five men among us, and one apprentice—not quite a year my senior—who used to sail out pretty regularly ten times a day on the average; two that went for whiskey only when some one invited them to drink, being too mean to treat themselves; and two more who never went at all.

In the afternoon, about half-past three, we had a cake-lunch, supplied by Uncle Jack Gridder, an old, crippled, superannuated ship carpenter. No one else was ever allowed to come in competition with our caterers. Let a foreign candy-board or cake basket make their appearance inside the gates of the yard, and they would get shipped out of that directly.

At about five o'clock p.m., always, Johnnie used to put in his second appearance; and then, having expended money in another stick or two of candy, and ten minutes in its consumption, we were ready to drive away again until sundown; then home to supper.

Less well-ordered in their daily pleasures, the shoemakers in Lynn, Massachusetts, nevertheless surrounded their way of work with a way of life. The former cobbler David Johnson recorded in minute detail in *Sketches of Old Lynn* how fishermen and farmers retained settled ways first as part-time shoemakers in small shops behind their homes. The language of the sea was adapted to the new craft:

> There were a good many sea phrases, or "salt notes" as they were called, used in the shops. In the morning one would hear, "Come Jake, hoist the sails," which simply was a call to roll up the curtains. . . . If debate ran high upon some exciting topic, some veteran would quietly remark, "Squally, squally, today. Come better *luff* and bear away."

At times a shoemaker read from a newspaper to other men at work. Festivals, fairs, games ("trolling the tog"), and excursions were common rituals among the Lynn cobblers. So was heavy drinking with the bill often incurred by "the one who made the most or the fewest shoes, the best or the poorest." That man "paid 'the scot.'" "These were the days," Johnson reminded later and more repressed New England readers, "when temperance organizations were hardly known."[26]

Despite the profound economic changes that followed the American Civil War, Gilded Age artisans did not easily shed stubborn and time-honored work habits. Such work habits and the life-styles and subcultures related to them retained a vitality long into these industrializing decades.

[26] Thompson, "Time, Work-Discipline, and Industrial Capitalism," 73; "A Workingman's Recollections of America," *Knight's Penny Magazine*, 1 (1846): 97–112; Richard D. Trevellick, in *Fincher's Trades Review*, n.d., reprinted in George E. McNeill, ed., *The Labor Movement: the Problem of To-day* (New York, 1887), 341–42; David Johnson, *Sketches of Old Lynn* (Lynn, 1880), 30–31, 36–49. The relationship between drink, work, and other artisanal communal activities was described inadvertently in unusual detail for dozens of British crafts and trades on nearly every page of John Dunlop's *The Philosophy of Artificial and Compulsory Drinking Usage in Great Britain and Ireland* (6th ed.; London, 1839), a 331-page temperance tract. There is good reason to believe that the craft customs described in this volume were known to American artisans and workers, too.

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Not all artisans worked in factories, but some that did retained traditional craft skills. Mechanization came in different ways and at different times to diverse industries. Samuel Gompers recollected that New York City cigarmakers paid a fellow craftsman to read a newspaper to them while they worked, and Milwaukee cigarmakers struck in 1882 to



Fig. 8. "Reader" in a cigar factory (probably in New York City), 1909. Photograph by Lewis W. Hine. Courtesy George Eastman House Collection.

retain such privileges as keeping (and then selling) damaged cigars and leaving the shop without a foreman's permission. "The difficulty with many cigarmakers," complained a New York City manufacturer in 1877, "is this. They come down to the shop in the morning; roll a few cigars and then go to a beer saloon and play pinnocio or some other game, . . . working probably only two or three hours a day." Coopers felt new machinery "hard and insensate," not a blessing but an evil that "took a great deal of joy out of life" because machine-made barrels undercut a subculture of work and leisure. Skilled coopers "lounged about" on Saturday (the regular pay day), a "lost day" to their employers. A historian of American cooperage explained:

> Early on Saturday morning, the big brewery wagon would drive up to the shop. Several of the coopers would club together, each paying his proper share, and one of them would call out the window to the driver, "Bring me a Goose Egg," meaning a half-barrel of beer. Then others would buy "Goose Eggs," and there

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would be a merry time all around. . . . Little groups of jolly fellows would often sit around upturned barrels playing poker, using rivets for chips, until they had received their pay and the "Goose Egg" was dry.

Saturday night was a big night for the old-time cooper. It meant going out, strolling around the town, meeting friends, usually at a favorite saloon, and having a good time generally, after a week of hard work. Usually the good time continued over into Sunday, so that on the following day he usually was not in the best of condition to settle down to the regular day's work.

Many coopers used to spend this day [Monday] sharpening up their tools, carrying in stock, discussing current events, and in getting things in shape for the big day of work on the morrow. Thus, "Blue Monday" was something of a tradition with the coopers, and the day was also more or less lost as far as production was concerned.

"Can't do much today, but I'll give her hell tomorrow," seemed to be the Monday slogan. But bright and early Tuesday morning, "Give her hell" they would, banging away lustily for the rest of the week until Saturday which was pay day again, and its thoughts of the "Goose Eggs."

Such traditions of work and leisure—in this case, a four-day work week and a three-day weekend—angered manufacturers anxious to ship goods as much as it worried sabbatarians and temperance reformers. Conflicts over life- and work-styles occurred frequently and often involved control over the work process and over time. The immigrant Staffordshire potters in Trenton, New Jersey, worked in "bursts of great activity" and then quit for "several days at a time." "Monday," said a manufacturer, "was given up to debauchery." After the potters lost a bitter lockout in 1877 that included torchlight parades and effigy burnings, the *Crockery and Glass Journal* mockingly advised:

Run your factories to please the crowd. . . . Don't expect work to begin before 9 a.m. or to continue after 3 p.m. Every employee should be served hot coffee and a boquet at 7 a.m. and allowed the two hours to take a free perfumed bath. . . . During the summer, ice cream and fruit should be served at 12 p.m. to the accompaniment of witching music.

Hand coopers (and potters and cigarmakers, among others) worked hard but in distinctly preindustrial styles. Machine-made barrels pitted modernizing technology and modern habits against traditional ways. To the owners of competitive firms struggling to improve efficiency and cut labor costs, the Goose Egg and Blue Monday proved the laziness and obstinacy of craftsmen as well as the tyranny of craft unions that upheld venerable traditions. To the skilled cooper, the long weekend symbolized a way of work and life filled with almost ritualistic meanings. Between 1843 and 1893, compromise between such conflicting interests was hardly possible.[27]

[27] Samuel Gompers, *Seventy Years of Life and Labor*, (New York, 1925), 1: 42–53, 63–82; Thomas Gavett, *Development of the Labor Movement in Milwaukee* (Madison, 1965), 43 ff.; New York *Herald*, Nov. 17, 1877; Franklin E. Coyne, *The Development of the Cooperage Industry in the United States* (Chicago, 1940), 7–26 but especially 21–22; *Crockery and Glass Journal*, n.d., reprinted in *Labor Standard* (N.Y.), Sept. 9, 1877; Frank Thistlethwaite, "Atlantic Migration of the Pottery Industry," *Economic History Review*, 10 (1957–58): 264–73.

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Settled premodern work habits existed among others than those employed in nonfactory crafts. Owners of already partially mechanized industries complained of them, too. "Saturday night debauches and Sunday carousels though they be few and far between," lamented the *Age of Steel* in 1882, "are destructive of modest hoardings, and he who indulges in them will in time become a striker for higher wages." In 1880 a British steel worker boasted that native Americans never would match immigrants in their skills: "'adn't the 'ops, you know." Manufacturers, when able, did not hesitate to act decisively to end such troubles. In Fall River new technology allowed a print cloth manufacturer to settle a long-standing grievance against his stubborn mule spinners. "On Saturday afternoon after they had gone home," a boastful mill superintendent later recollected, "we started right in and smashed a room full of mules with sledge hammers. . . . On Monday morning, they were astonished to find that there was not work for them. That room is now full of ring frames run by girls." Woolen manufacturers also displaced handjack spinners with improved machinery and did so because of "the disorderly habits of English workmen. Often on a Monday morning, half of them would be absent from the mill in consequence of the Sunday's dissipation." Blue Monday, however, did not entirely disappear. Paterson artisans and factory hands held a May festival on a Monday each year ("Labor Monday") and that popular holiday soon became state law, the American Labor Day. It had its roots in earlier premodern work habits.[28]

The persistence of such traditional artisan work habits well into the nineteenth century deserves notice from others besides labor historians, because those work habits did not exist in a cultural or social vacuum. If modernizing technology threatened and even displaced such work patterns, diverse nineteenth-century subcultures sustained and nourished them. "The old nations of the earth creep on at a snail's pace," boasted Andrew Carnegie in *Triumphant Democracy* (1886), "the Republic thunders past with the rush of an express." The articulate steelmaster, however, had missed the point. The very rapidity of the economic changes occurring in Carnegie's lifetime meant that many, unlike him, lacked the time, historically, culturally, and psychologically, to be separated or alienated from settled ways of work and life and from relatively fixed beliefs. Continuity not consensus counted for much in explaining working-class and especially artisan behavior in those decades that witnessed the coming of the factory and the radical transformation of American society. Persistent work habits were one example of that significant continuity. But these elements of continuity were often revealed among nineteenth-century American workers cut off by birth from direct contact with the

[28] *Age of Steel*, Aug. 5, 1882 (courtesy of Lynn Mapes); Berthoff, *British Immigrants in Industrial America*, 54–55, 146; announcement of "Great Festival" on "Labor Monday," Paterson *Labor Standard*, May 29, 1880.

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preindustrial American past, a fact that has been ignored or blurred by the artificial separation between labor history and immigration history. In Gilded Age America (and afterwards in the Progressive Era despite the radical change in patterns of immigration), working-class and immigration history regularly intersected, and that intermingling made for powerful continuities. In 1880, for example, 63 of every 100 Londoners were native to that city, 94 coming from England and Wales, and 98 from Great Britain and Ireland. Foreign countries together contributed only 1.6 per cent to London's massive population. At that same moment, more than 70 of every 100 persons in San Francisco (78), St. Louis (78), Cleveland (80), New York (80), Detroit (84), Milwaukee (84), and Chicago (87) were immigrants or the children of immigrants, and the percentage was just as high in many smaller American industrial towns and cities. "Not every foreigner is a workingman," noticed the clergyman Samuel Lane Loomis in 1887, "but in the cities, at least, it may almost be said that every workingman is a foreigner." And until the 1890s most immigrants came from Northern and Western Europe, French- and English-speaking Canada, and China. In 1890, only three per cent of the nation's foreign-born residents—290,000 of 9,200,000 immigrants—had been born in Eastern or Southern Europe. (It is a little recognized fact that most North and West European immigrants migrated to the United States after, not before, the American Civil War.) When so much else changed in the industrializing decades, tenacious traditions flourished among immigrants in ethnic subcultures that varied greatly among particular groups and according to the size, age, and location of different cities and industries. ("The Irish," Henry George insisted, "burn like chips, the English like logs.") Class and occupational distinctions within a particular ethnic group made for different patterns of cultural adaptation, but powerful subcultures thrived among them all.[29]

Immiserization and poverty cut deeply into these ethnic working-class worlds. In reconstructing their everyday texture there is no reason to neglect or idealize such suffering, but it is time to discard the notion that the large-scale uprooting and exploitative processes that accompanied industrialization caused little more than cultural breakdown and social anomie. Family, class, and ethnic ties did not dissolve easily. "Almost as a matter of definition," the sociologist Neil Smelzer has written, "we associate the factory system with the decline of the family and the onset of anonymity." Smelzer criticized such a view of early industrializing England, and it has just as little validity for nineteenth-century industrializing America. Family roles changed in important ways, and strain was widespread, but the immigrant working-class family held together.

[29] Andrew Carnegie quoted in Henry Pelling, *America and the British Left* (New York, 1957), 52; Samuel Lane Loomis, *Modern Cities and Their Religious Problems* (New York, 1887), 68–73; Henry George quoted in Carl Wittke, *Irish in America* (Baton Rouge, 1956), 193.

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Examination of household composition in sixteen census enumeration districts in Paterson in 1880 makes that clear for this predominantly working-class immigrant city, and while research on other ethnic working-class communities will reveal significant variations, the overall patterns should not differ greatly. The Paterson immigrant (and native white) communities were predominantly working class, and most families among

TABLE 1. MALE OCCUPATIONAL STRUCTURE AND HOUSEHOLD COMPOSITION BY ETHNIC GROUP, PATERSON, NEW JERSEY, 1880, ENUMERATION DISTRICTS 150–53, 161–72[a]

	British	*German*	*Irish*	*Native White*
Total Males 20 and Older	2090	927	2841	1461
Total Females 20 and Older	1941	804	3466	1689
Male Occupational Structure				
Unskilled Laborer	8.2%	9.8%	43.6%	20.8%
Skilled Worker	75.5%	64.3%	44.8%	62.5%
Nonlaborer	16.3%	25.9%	11.6%	16.7%
Household Composition				
Number of Kin-related Households	1402	686	2142	905
Number of Subfamilies[b]	117	41	158	125
Nuclear Households	73.9%	78.1%	73.1%	65.7%
Extended Households	13.5%	10.3%	13.6%	18.7%
Augmented Households[c]	14.6%	13.1%	15.3%	19.0%
Per cent of Households and Subfamilies with a Husband and/or Father Present	87.2%	91.6%	81.1%	78.9%

[a] I am indebted to Carol W. Allison for gathering the raw Paterson data from the 1880 federal manuscript census schedules.

[b] A subfamily is defined as a complete or incomplete nuclear family residing with another nuclear family.

[c] Augmented households include lodgers. The sum of nuclear, augmented, and extended households is greater than 100 per cent because some households included both relatives and lodgers and have been counted twice.

them were intact in their composition. For this population, at least (and without accounting for age and sex ratio differences between the ethnic groups), a greater percentage of immigrant than native white households included two parents. Ethnic and predominantly working-class communities in industrial towns like Paterson and in larger cities, too, built on these strained but hardly broken familial and kin ties. Migration to another country, life in the city, and labor in cost-conscious and ill-equipped factories and workshops tested but did not shatter what the anthropologist Clifford Geertz has described as primordial (as contrasted to civic) attachments, "the 'assumed' givens . . . of social existence: immediate contiguity and kin connections mainly, but beyond them, the

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givenness that stems from being born into a particular religious community, speaking a particular language, and following particular social patterns." Tough familial and kin ties made possible the transmission and adaptation of European working-class cultural patterns and beliefs to industrializing America. As late as 1888, residents in some Rhode Island mill villages still figured their wages in British currency. Common rituals and festivals bound together such communities. Paterson silk weavers had their Macclesfield wakes, and Fall River cotton-mill workers their Ashton wakes. British immigrants "banded together to uphold the popular culture of the homeland" and celebrated saints' days: St. George's Day, St. Andrew's Day, and St. David's Day. Even funerals retained an archaic flavor. Samuel Sigley, a Chartist house painter, had fled Ashton-under-Lyne in 1848, and built American trade unions. When his wife died in the late 1890s a significant ritual occurred during the funeral: some friends placed a chaff of wheat on her grave. Mythic beliefs also cemented ethnic and class solidarities. The Irish-American press, for example, gave Martin O'Brennan much space to argue that Celtic had been spoken in the Garden of Eden, and in Paterson Irish-born silk, cotton, and iron workers believed in the magical powers of that town's "Dublin Spring." An old resident remembered:

> There is a legend that an Irish fairy brought over the water in her apron from the Lakes of Killarney and planted it in the humble part of that town. . . . There were dozens of legends connected with the Dublin Spring and if a man drank from its precious depository . . . he could never leave Paterson [but] only under the fairy influence, and the wand of the nymph would be sure to bring him back again some time or other.

When a "fairy" appeared in Paterson in human form, some believed she walked the streets "as a tottering old woman begging with a cane." Here was a way to assure concern for the elderly and the disabled.[30]

Much remains to be studied about these cross-class but predominantly working-class ethnic subcultures common to industrializing America. Relations within them between skilled and unskilled workers, for example, remain unclear. But the larger shape of these diverse immigrant communities can be sketched. More than mythic beliefs and common work habits sustained them. Such worlds had in them what Thompson has called "working-class intellectual traditions, working-class community patterns,

[30] Neil Smelzer, *Social Change in the Industrial Revolution* (Chicago, 1959), 193; Clifford Geertz, *Old Societies and New States* (Glencoe, 1963), 109–10; Lillie B. Chace Wyman, "Studies in Factory Life," *Atlantic Monthly*, 62 (1888): 17–29, 215–21, 605–21 and 63 (1889): 68–79; Berthoff, *British Immigrants in Industrial America*, 147–81, *passim*; Paterson *Labor Standard*, Oct. 2, 1897; Thomas N. Brown, *Irish-American Nationalism* (Philadelphia, 1966), 32; Paterson *Evening News*, Oct. 27, 1900. Except for the fact that nuclear households declined greatly at the expense of households containing lodgers (augmented households), examination of the household composition among immigrant Jews and Italians in Lower Manhattan in 1905 shows that powerful familial and kin ties bound together later immigrant communities, too. The data are summarized briefly in table 3 (see appendix).

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and a working-class structure of feeling," and men with artisan skills powerfully affected the everyday texture of such communities. A model subculture included friendly and benevolent societies as well as friendly local politicians, community-wide holiday celebrations, an occasional library (the Baltimore Journeymen Bricklayer's Union taxed members one dollar a year in the 1880s to sustain a library that included the collected works of William Shakespeare and Sir Walter Scott's Waverley novels), participant sports, churches sometimes headed by a sympathetic clergy, saloons, beer-gardens, and concert halls or music halls and, depending upon circumstance, trade unionists, labor reformers, and radicals. The Massachusetts cleric Jonathan Baxter Harrison published in 1880 an unusually detailed description of one such ethnic, working-class institution, a Fall River music hall and saloon. About fifty persons were there when he visited it, nearly one-fourth of them young women. "Most of those present," he noticed, were "persons whom I had met before, in the mills and on the streets. They were nearly all operatives, or had at some time belonged to that class." An Englishman sang first, and then a black whose songs "were of many kinds, comic, sentimental, pathetic, and silly. . . . When he sang 'I got a mammy in the promised land,' with a strange, wailing refrain, the English waiter-girl, who was sitting at my table, wiped her eyes with her apron, and everybody was very quiet." Harrison said of such places in Fall River:

> All the attendants . . . had worked in the mills. The young man who plays the piano is usually paid four or five dollars per week, besides his board. The young men who sing receive one dollar per night, but most of them board themselves. . . . The most usual course for a man who for any reason falls out of the ranks of mill workers (if he loses his place by sickness or is discharged) is the opening of a liquor saloon or drinking place.

Ethnic ties with particular class dimensions sometimes stretched far beyond local boundaries and even revealed themselves in the behavior of the most successful practitioners of Gilded Age popular culture. In 1884, for example, the pugilist John L. Sullivan and the music-hall entertainers Harrigan and Hart promised support to striking Irish coal miners in the Ohio Hocking Valley. Local ties, however, counted for much more and had their roots inside and outside of the factory and workshop. Soon after Cyrus H. McCormick, then twenty-one, took over the management of his father's great Chicago iron machinery factory (which in the early 1880s employed twelve hundred men and boys), a petition signed by "Many Employees" reached his hands:

> It only pains us to relate to you . . . that a good many of our old hands is not here this season and if Mr. Evarts is kept another season a good many more will leave. . . . We pray for you . . . to remove this man. . . . We are treated as though we were dogs. . . . He has cut wages down so low they are living on nothing but

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bread. . . . We can't talk to him about wages if we do he will tell us to go out side the gate. . . . He discharged old John the other day he has been here seventeen years. . . . There is Mr. Church who left us last Saturday he went about and shook hands with every old hand in the shop . . . this brought tears to many mens eyes. He has been here nineteen years and has got along well with them all until he came to Mr. Evarts the present superintendent.

Artisans, themselves among those later displaced by new technology, signed this petition, and self-educated artisans (or professionals and petty enterprisers who had themselves usually risen from the artisan class) often emerged as civic and community leaders. "Intellectually," Jennie Collins noticed in Boston in the early 1870s, "the journeymen tailors . . . are ever discussing among themselves questions of local and national politics, points of law, philosophy, physics, and religion."[31]

Such life-styles and subcultures adapted and changed over time. In the Gilded Age piece rates in nearly all manufacturing industries helped reshape traditional work habits. "Two generations ago," said the Connecticut Bureau of Labor Statistics in 1885, "time-work was the universal rule." "Piece-work" had all but replaced it, and the Connecticut Bureau called it "a moral force which corresponds to machinery as a physical force." Additional pressures came in traditional industries such as shoe, cigar, furniture, barrel, and clothing manufacture, which significantly mechanized in these years. Strain also resulted where factories employed large numbers of children and young women (in the 1880 manuscript census 49.3 per cent of all Paterson boys and 52.1 per cent of all girls aged eleven to fourteen had occupations listed by their names) and was especially common among the as yet little-studied pools of casual male laborers found everywhere. More than this, mobility patterns significantly affected the structure and the behavior of these predominantly working-class communities. A good deal of geographic mobility, property mobility (home ownership), and occupational mobility (skilled status in new industries or in the expanding building trades, petty retail enterprise, the professions, and public employment counted as the most important ways to advance occupationally) reshaped these ethnic communities as Stephan Thernstrom and others have shown. But so little is yet known about the society in which such men and women lived and about the cultures which had produced them that it is entirely premature to infer "consciousness" (beliefs and values) only from mobility rates. Such patterns and rates of mobility, for example, did not entirely shatter working-class capacities for self-protection. The fifty-year period between 1843 and 1893 was not conducive to permanent, stable trade unions,

[31] Thompson, *Making of the English Working Class*, 194; Richard T. Ely, *Labor Movement in America* (New York, 1886), 125; Jonathan Baxter Harrison, *Certain Dangerous Tendencies in American Life and Other Essays* (Boston, 1880), 178–88; *National Labor Tribune* (Pittsburgh), Dec. 13, 1884; Robert Ozanne, *Century of Labor-Management Relations at McCormick and International Harvester* (Madison, 1967), 10–28; Collins, *Nature's Aristocracy*, 94.

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but these decades were a time of frequent strikes and lockouts and other forms of sustained conflict.[32]

Not all strikes and lockouts resulted in the defeat of poorly organized workers. For the years 1881 to 1887, for example, the New Jersey Bureau of Labor Statistics collected information on 890 New Jersey industrial disputes involving mostly workers in the textile, glass, metal, transportation, and building trades: six per cent ended in compromise settlements; employers gained the advantage in forty per cent; strikers won the rest (fifty-four per cent). In four of five disputes concerning higher wages and shorter hours, New Jersey workers, not their employers, were victorious. Large numbers of such workers there and elsewhere were foreign-born or the children of immigrants. More than this, immigrant workers in the mid–1880s joined trade unions in numbers far out of proportion to their place in the labor force. Statistical inquiries by the Bureau of Labor Statistics in Illinois in 1886 and in New Jersey in 1887 make this clear. Even these data may not have fully reflected the proclivity of immigrants to seek self-protection. (Such a distortion would occur if, for example, the children of immigrants apparently counted by the bureaus as native-born had remained a part of the ethnic subcultures into which they had been born and joined trade unions as regularly as the foreign-born.) Such information from Illinois and New Jersey suggests the need to treat the meaning of social mobility with some care. So does the sketchy outline of Hugh O'Donnell's career. By 1892, when he was twenty-nine years old, he had already improved his social status a great deal. Before the dispute with Andrew Carnegie and Henry Clay Frick culminated in the bitter Homestead lockout that year, O'Donnell had voted Republican, owned a home, and had in it a Brussels carpet and even a piano. Nevertheless

TABLE 2. ORGANIZED WORKERS, MALE WHITES IN NONAGRICULTURAL PURSUITS, ILLINOIS (1886) AND NEW JERSEY (1887)

	Illinois 1886		*New Jersey 1887*	
Nativity	*Breadwinners*	*Organized*	*Breadwinners*	*Organized*
Number				
Native-born	423,290	25,985	243,093	24,463
Foreign-born	308,595	57,163	137,385	26,704
Per cent				
Native-born	57.8%	31.3%	63.9%	47.8%
Foreign-born	42.2%	68.7%	36.1%	52.2%

[32] *Connecticut Bureau of Labor Statistics, First Annual Report, 1885* (Hartford, 1885), 70–73; Stephan Thernstrom, *Poverty and Progress, Social Mobility in a Nineteenth Century City* (Cambridge, 1964), *passim;* and Thernstrom and Richard Sennett, eds., *Nineteenth Century Cities* (New Haven, 1969), *passim.*

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this Irish-American skilled worker led the Homestead workers and was even indicted under a Civil War treason statute never before used. The material improvements O'Donnell had experienced mattered greatly to him and suggested significant mobility, but culture and tradition together with the way in which men like O'Donnell interpreted the transformation of Old America defined the value of those material improvements and their meaning to him.[33]

Other continuities between 1843 and 1893 besides those rooted in artisan work habits and diverse ethnic working-class subcultures deserve brief attention as important considerations in understanding the behavior of artisans and other workers in these decades. I have suggested in other writings that significant patterns of opposition to the ways in which industrial capitalism developed will remain baffling until historians re-examine the relationship between the premodern American political system and the coming of the factory along with the strains in premodern popular American ideology shared by workers and large numbers of successful self-made Americans (policemen, clergymen, politicians, small businessmen, and even some "traditional" manufacturers) that rejected the legitimacy of the modern factory system and its owners.[34] One strain of thought common to the rhetoric of nineteenth-century immigrant and native-born artisans is considered here. It helps explain their recurrent enthusiasm for land and currency reform, cooperatives, and trade unions. It was the fear of dependence, "proletarianization," and centralization, and the worry that industrial capitalism threatened to transform "the Great Republic of the West" into a "European" country. In 1869, the same year that saw the completion of the transcontinental railroad, the chartering of the Standard Oil Company, the founding of the Knights of Labor, and the dedication of a New York City statue to Cornelius Vanderbilt, some London workers from Westbourne Park and Notting Hill petitioned the American Ambassador for help to emigrate. "Dependence," they said of Great Britain, "not independence, is inculcated. Hon. Sir, this state of things we wish to fly from . . . to become citizens of that great Republican country, which has no parallels in the world's history." Such men had a vision of Old America, but it was not a new vision. Industrial transformation between 1840 and 1890 tested and redefined that vision.

[33] Table on New Jersey and Illinois trade union membership in Isaac Hourwich, *Immigration and Labor: The Economic Aspects of European Immigration to the United States* (New York, 1912), 524; Leon Woolf, *Lockout: the Story of the Homestead Strike of 1892* (New York, 1965), 187–88.

[34] See, for examples, H. G. Gutman, "The Worker's Search for Power: Labor in the Gilded Age," in H. Wayne Morgan, ed., *The Gilded Age: A Reappraisal* (Syracuse, 1963), 38–68; Gutman, "Protestantism and the American Labor Movement: The Christian Spirit in the Gilded Age," *American Historical Review*, 72 (1966–67): 74–101; Gutman, "Class, Status, and Community Power in Nineteenth Century American Industrial Cities: Paterson, New Jersey, a Case Study," in Frederic C. Jaher, ed., *Age of Industrialism: Essays in Social Structure and Cultural Values* (New York, 1968), 263–87.

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Seven years after their visit, the New York *Labor Standard,* then edited by an Irish socialist, bemoaned what had come over the country: "There was a time when the United States was the workingman's country, . . . the land of promise for the workingman. . . . We are now in an *old country.*" This theme recurred frequently as disaffected workers, usually self-educated artisans, described the transformation of premodern America. "America," said the Detroit *Labor Leaf,* "used to be the land of promise to the poor. . . . The Golden Age is indeed over—the Age of Iron has taken its place. The iron law of necessity has taken the place of the golden rule." We need not join in mythicizing preindustrial American society in order to suggest that this tension between the old and the new helps give a coherence to the decades between 1843 and 1893 that even the trauma of the Civil War does not disturb.[35]

As early as the 1830s, the theme that industrialism promised to make over the United States into a "European" country had its artisan and working-class advocates. Seth Luther then made this clear in his complaint about "gentlemen" who "exultingly call LOWELL the Manchester of America" and in his plea that the Bunker Hill monument "stand *unfinished,* until the time passes away when aristocrats talk about mercy to mechanics and laborers, . . . until our rights are acknowledged." The tensions revealed in labor rhetoric between the promises of the Republic and the practices of those who combined capital and technology to build factories continued into the 1890s. In 1844 New England shoemakers rewrote the Declaration of Independence to protest that the employers "have robbed us of certain rights," and two years later New England textile workers planned without success a general strike to start on July 4, 1846, calling it "a second Independence Day." The great 1860 shoemakers' strike in Lynn started on George Washington's birthday, a celebration strikers called "sacred to the memory of one of the greatest men the world has ever produced." Fear for the Republic did not end with the Civil War. The use of state militia to help put down a strike of Northeastern Pennsylvania workers in 1874 caused *Equity,* a Boston labor weekly, to condemn the Erie Railroad as "the George III of the workingman's movement" and "the Government of Pennsylvania" as "but its parliament." ("Regiments," it added, "to protect dead things.")[36]

Such beliefs, not the status anxieties of Progressive muckrakers and New Deal historians, gave rise to the pejorative phrase "robber baron." Discontented Gilded Age workers found in that phrase a way to summarize their worries about dependence and centralization. "In America," exploded

[35] *Reynold's Newspaper* (London), Mar. 28, 1869; *Labor Standard* (N.Y.), May 6, 1876; Detroit *Labor Leaf,* Sept. 30, 1885.

[36] Luther, *Address to the Workingmen of New England, passim;* Ware, *Industrial Worker, 1840–1860,* 38–48; Philip S. Foner, *History of the Labor Movement in the United States* (New York, 1947), 1: 202–09, 241–45, 292; *Equity* (Boston), 1 (1874), quoted in James Dombrowski, *Early Days of Christian Socialism* (New York, 1936), 81.

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the *National Labor Tribune* in 1874, "we have realized the ideal of republican government at least in form." "America," it went on, "was the star of the political Bethlehem which shone radiantly out in the dark night of political misrule in Europe. The masses of the old world gazed upon her as their escape." Men in America could be "their own rulers"; "no one could or should become their masters." But industrialization had created instead a nightmare: "These dreams have not been realized. . . . The working people of this country . . . suddenly find capital as rigid as an absolute monarchy." Two years later, the same Pittsburgh labor weekly asked, "Shall we let the gold barons of the nineteenth century put iron collars of ownership around our necks as did the feudal barons with their serfs in the fourteenth century?" The rhetoric surrounding the little-understood 1877 railroad strikes and riots summed up these fears. Critics of the strikers urged repressive measures such as the building of armories in large cities and the restriction of the ballot, and a few, including Elihu Burritt, even favored importing "British" institutions to the New World. But the disorders also had their defenders, and a strain in their rhetoric deserves notice. A radical Massachusetts clergyman called the strikers "the lineal descendants of Samuel Adams, John Hancock, and the Massachusetts yeomen who began so great a disturbance a hundred years ago . . . only now the kings are money kings and then they were political kings." George McNeill, a major figure in the nineteenth-century labor movement and later a founder of the American Federation of Labor, denied that the Paris Commune had come to America: "The system which the pilgrims planted here has yet a residue of followers. No cry of 'commune' can frighten the descendants of the New England commune. This is the COMMONWEALTH, not the *Class* wealth, of Massachusetts." A discharged Pittsburgh brakeman put it differently in blaming the violence on a general manager who treated the railroad workers "no better than the serfs of Great Britain, sir, . . . introduced into this country a lot of English ideas and customs, [and] made our men wear uniforms and traveling bags." "A uniform," he worried, "constantly reminds them of their serfdom, and I for one would rather remain out of work than wear one." An amazed reporter wondered how this man could "assert his rights as a free born American, even if in so doing himself and family starved."[37]

This Pittsburgh brakeman revealed values that persisted throughout the decades of industrialization, that expressed themselves most commonly in the rhetoric and behavior of artisans and skilled workers, and that worried other influential Americans besides railroad magnates and industrial manufacturers. In 1896 an army officer won a prize for writing

[37] *National Labor Tribune* (Pittsburgh), Dec. 12, 1874, and Oct. 14, 1876; Jesse Jones, "Railroad Strike of 1877," and George McNeill, "An Address," *Labor Standard* (N.Y.), Aug. 26, and Sept. 30, 1877; Robert, Pittsburgh dispatch, Chicago *Inter-Ocean*, Sept. 11, 1877.

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the best essay submitted to the *Journal of the Military Service Institutions of the United States.* Theodore Roosevelt helped to judge the contest. The officer insisted that "discipline" needed to be more rigorous in an American as opposed to a European army. Even though he knew little about European societies, his insistence that "means of discipline are entirely artificial productions of law" in the United States counted as a profound insight into a social condition that plagued industrialists and sparked frequent discontent among skilled and other workers in industrializing America:

> Discipline should be as a rod of iron. It may seem hopelessly illogical to claim that the army of a free people needs to be kept in stricter discipline than any other army, with wider space between the officers and the enlisted men, yet there are natural reasons why it should be so. The armies of Europe are drawn from people who for countless generations have lived under monarchical institutions and class government, where every man is born and bred to pay homage to some other man, and the habit of subordination to the will of another is a matter of heredity. It is natural that when such a man finds himself in the army he is not only amenable to discipline, but any relaxation on the part of the officer would be accepted as a matter of grace.
>
> With us these conditions are reversed. Every man is born and bred in the idea of equality, and means of discipline are entirely artificial productions of law, not only without support from traditional habit, but they have that habit to overcome, and familiarity on the part of the officer would breed contempt of authority.

Two decades earlier, the London editor of the *Industrial Review* and increasingly conservative British trade-union leader, George Potter, posed the same problem somewhat differently. The disorders incident to the 1877 railroad strikes convinced him that Americans then lived through an earlier stage of English history, before "habit" had "begotten" men to "use their combinations peaceably and wisely." "The state of things that existed then in England," Potter insisted, "exists now in the United States. It was at one time believed that this was impossible within the borders of the great Republic, but it has proved itself wrong." Potter believed that the widespread violence in 1877 had been caused by men "suddenly or newly brought together to defend an interest" and therefore lacking "that wisdom of method that time and experience develop." But Potter was wrong. The men who quit work in 1877 (and before and after that) included many deeply rooted in traditional crafts and worried that the transformation of the American social and economic structure threatened settled ways of work and life and particular visions of a just society. Their behavior—in particular the little-understood violence that accompanied the strikes (including the burning and destruction of the Pennsylvania Railroad's Pittsburgh yards and equipment)—makes this clear. It had specific purposes and was the product of long-standing

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grievances that accompanied the transformation of Old America into New America.[38]

QUITE DIVERSE PATTERNS of collective working-class behavior (some of them disorderly and even violent) accompanied the industrialization of the United States, and certain of them (especially those related to artisan culture and to peasant and village cultures still fresh to factory labor and to the machine) deserve brief attention. Characteristic European forms of "premodern" artisan and lower-class protest in the United States occurred before (prior to 1843), during (1843–93), and after (1893–1919) the years when the country "modernized." The continuing existence of such behavior followed from the changing composition of the working-class population. Asa Briggs's insistence that "to understand how people respond to industrial change it is important to examine what kind of people they were at the beginning of the process" and "to take account of continuities as well as new ways of thinking," poses in different words the subtle interplay between culture and society that is an essential factor in explaining working-class behavior. Although their frequency remains the subject for much further detailed study, examples of premodern working-class behavior abound for the entire period from 1815 to 1919, and their presence suggests how much damage has been done to the past American working-class experiences by historians busy, as R. H. Tawney complained more than half a century ago, "dragging into prominence forces which have triumphed and thrusting into the background those which have been swallowed up." Attention is briefly given to three types of American artisan and working-class behavior explored in depth and with much illumination by European social historians ("church-and-king" crowds, machine-breaking, and food riots) and to the presence in quite different working-class protests of powerful secular and religious rituals. These occurred over the entire period under examination, not just in the early phases of industrial development.[39]

Not much is yet known about premodern American artisan and urban lower-class cultures, but scattered evidence suggests a possible American variant of the European church-and-king phenomenon. Although artisan and lower-class urban cultures before 1843 await their historians, popular street disorders (sometimes sanctioned by the established authorities) happened frequently and increasingly caused concern to the premodern

[38] Major George Wilson, "The Army: Its Employment During Times of Peace and the Necessity for Its Increase," *Journal of the Military Service Institution of the United States*, 18 (1896): 8–9; George Potter, "The American Labour Riots," *Industrial Review* (London), Aug. 4, 1877, p. 9.

[39] Asa Briggs, review of Thompson, *Making of the English Working Class*, in *Labor History*, 6 (1965): 84–91; R. H. Tawney, *Agrarian Problem in the Sixteenth Century* (London, 1912), 177.

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elite classes. Street gangs, about which little is yet known except the suggestion that some had as members artisans (not just casual or day laborers) and were often organized along ethnic lines, grew more important in the coastal and river towns after 1830. New York City, among other towns, had its Fly Boys, Chichesters, Plug Uglies, Buckaroos, and Slaughterhouse Gangs, and their violence against recent immigrants provoked disorderly counterthrusts. Political disorders on election days, moreover, were apparently well-organized and may have involved such gangs. The recurrence of such disorders through the pre-Civil War decades (including the nativist outbursts in nearly all major Northern and Southern cities in the 1850s) may have meant that local political parties, in their infancy, served as the American substitute for the King and the Church, a third party "protecting" artisans and even day laborers from real and imagined adversaries and winning clanlike loyalty. Although the testimony of Mike Walsh, a Tammany leader and later the publisher of the *Police Gazette,* must be read with care, he suggested an interesting relationship between the decline of premodern lower-class entertainments and the rise of modern political "machines." Election politics, Walsh noted in the *Subterranean,* saw "the Goth-and-Vandal-like eruption of the shirtless and unwashed democracy" which Walsh connected to the disappearance of popular lower-class entertainments. A "gloomy, churlish, money-worshipping . . . spirit" had "swept nearly all the poetry out of the poor man's sphere," said the editor-politician. "Ballad-singing, street dancing, tumbling, public games, all are either prohibited or discountenanced, so that Fourth of July and election sports alone remain." Workers flocked to political clubs and labored hard for a party to "get a taste of the equality which they hear so much preached, but never, save there, see even partially practiced." If Walsh's insight has merit, political parties quite possibly competed with early craft unions in adapting older forms of popular entertainment and ritual to changing needs. That process, once started, had a life beyond the early years of the premodern political party and continued as the composition of the working-class changed. The ethnic political "boss" created a new dependence that exploited well-understood class feelings and resentments but blunted class consciousness. The relationship, however, was not simple, and in the 1880s the socialist Joseph P. McDonnell exploited that same relationship to convince local New Jersey politicians to respond to pressures from predominantly immigrant workers and thereby to pioneer in the passage of humane social legislation, a process that began well before the stirring of the middle- and upper-class conscience in Progressive America.[40]

[40] Mike Walsh, *Subterranean* (N.Y., n.d.), quoted in M. R. Werner, *Tammany Hall* (New York, 1932), 49–51 (courtesy of Paul Weinbaum). On gangs, nativism, politics, and antebellum street violence, see A. F. Harlow, *Old Bowery Days* (New York, 1931), *passim; Protestant Crusade, 1800–1860* (New York, 1938), *passim;* and McNeill, *Labor Movement,* 344. The ways in which McDonnell used machine politics and politicians to push social reform in the 1880s

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Available evidence does not yet indicate that machine-breaking of the "Luddite" variety was widespread in the United States. There are suggestive hints in reports that Ohio farm laborers burnt and destroyed farm machinery in 1878 and that twenty years later in Buffalo a crowd of Polish common day laborers and their wives rioted to break a street-paving machine, but the only clear evidence found of classic machine-breaking occurred early in the Civil War among rural blacks in the South Carolina Sea Islands, who resisted Yankee missionary and military efforts to make them plant cotton instead of corn and therefore broke up cotton gins and hid the iron work. "They do not see the use of cotton," said a Northern female school teacher, and a Yankee entrepreneur among them added that "nothing was more remote from their shallow pates than the idea of planting cotton for 'white-folks' again." (Some time later, this same man ordered a steam-run cotton gin. "This engine," he confided, "serves as a moral stimulus to keep the people at work at their hand-gins, for they want to gin all the cotton by hand, and I tell them if they don't by the middle of January I shall get it by steam.") If white workers rarely broke machines to protest their introduction, they sometimes destroyed the product of new technology. In the early 1830s Brooklyn ropemakers paraded a "hated machine" through town and then "committed to the flames" its product. Theirs was not an irrational act. They paid for the destroyed hemp, spun "a like quantity" to allow the machine's owner to "fulfill his engagement for its delivery," and advertised their product in a newspaper, boasting that its quality far surpassed machine-made rope "as is well known to any practical ropemaker and seaman." Silk weavers in the Hudson River towns of New Jersey broke looms in 1877 but only to prevent production during a strike. A more common practice saw the destruction of the product of labor or damage to factory and mining properties to punish employers and owners. Paterson silk weavers regularly left unfinished warps to spoil in looms. Crowds often stoned factories, burned mine tipples, and did other damage to industrial properties (as in the bitter Western Pennsylvania coke strikes between 1884 and 1894) but mostly to protest the hiring of new hands or violence against them by "police." Construction gangs especially in railroad work also frequently destroyed property. In 1831, between two and three hundred construction workers, mostly Irish, punished an absconding contractor by "wantonly" tearing up track they built. Similar penalties were meted out by Italian construction gangs between 1880 and 1910 and by unorganized railroad workers, mostly native-born repairmen and trainmen, between 1850 and 1880, who

are described in Gutman, "Class, Status, and the Gilded Age Radical: The Case of a New Jersey Socialist," in a work currently in press, Gutman and Gregory S. Kealey, eds., *Many Pasts: Readings in American Social History* (Englewood Cliffs, 1973), vol. 2.

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tore up track, spiked switches, stole coupling links and pins, and did other damage to protest changing work rules or to collect back wages.[41]

"Luddism" may have been rare, but classic "European" food riots occurred in the United States, and two in New York City—the first in 1837 and the second in 1902—that involved quite different groups of workers are briefly examined to illustrate the ways in which traditional cultural forms and expectations helped shape working-class behavior. (Other evidence of similar disorders, including the Confederate food riots led by white women in Mobile, Savannah, and Richmond, await careful study.) In February 1837, thousands gathered in City Hall Park to protest against "monopolies" and rising food prices. Some months before that park had witnessed yet another demonstration against the conspiracy trial of twenty-five striking journeymen tailors. In their rhetoric the protesters identified the trial with the betrayal of the premodern "Republic." "Aristocrats" had robbed the people of "that liberty bequeathed to them, as a sacred inheritance by their revolutionary sires" and "so mystified" the laws that "men of common understanding cannot unravel them." "What the people thought was liberty, bore not a semblance to its name." Resolutions compared the tailors to that "holy combination of that immortal band of

[41] *Labor Standard* (N.Y.), Sept. 28, 1878; Edward S. Abdy, *Journal of a Residence and Tour in the United States from April 1833 to October 1834* (London, 1838), 1: 77–79; Gutman, "Class, Status, and Community Power"; Pennsylvania Bureau of Labor Statistics, *Fifteenth Annual Report, 1887* (Harrisburg, 1888), F1–F18 and *Nineteenth Annual Report, 1891* (Harrisburg, 1892), D1–D18; *Niles' Weekly Register*, 40 (1831): 338–39; New York *Tribune*, May 2, 1857; *John Swinton's Paper* (N.Y.), Feb. 24, 1884; New York *Tribune*, Oct. 21, 1893; *New York State Board of Mediation and Arbitration, Eleventh Annual Report, 1898* (New York, 1899), 139–42; Gutman, "Trouble on the Railroads in 1873–1874," *Labor History*, 2 (1961): 215–35. The materials on the Sea Island blacks are found in Laura Towne, *Letters and Diaries of Laura S. Towne 1862–1884, Written from the Sea Islands of South Carolina*, ed. Rupert S. Holland (Cambridge, Mass., 1910), 16–17, 20–21; Elizabeth Ware Pearson, ed., *Letters from Port Royal, 1862–1868* (Boston, 1906), 221–22, 236–37, 250; Willie Lee Rose, *Port Royal Experiment: Rehearsal for Reconstruction* (Indianapolis, 1964), 141; Jane and William Pease, *Black Utopias* (Madison, 1963), 134, 143, 149–50. Although American blacks are not included in these pages, the behavior and thought of rural and urban blacks fits the larger patterns suggested here in a special way. Their experiences first as slaves and then as dependent laborers in the rural South as well as in the industrial North (where most manufacturing industries remained closed to them until the First World War) distinguished most lower-class blacks from all immigrant and native white workers. In still little-understood but profoundly important ways enslavement followed by racial exclusion sustained among blacks a culture that despite change remained preindustrial for more than merely two or three generations. Despite this significant difference, similarities in behavior between blacks and native and immigrant white workers can be noticed. Visitors to the Richmond tobacco factories in the 1850s found industrial slaves there who practiced "Blue Monday." Joseph C. Roberts, *The Story of Tobacco in America* (New York, 1949), 86–91. Blacks themselves made comparisons to whites who shared difficult premodern rural experiences: "I have never heard any songs like those [slave songs] anywhere since I left slavery, except when in Ireland. . . . It was during the famine of 1845–1846." Frederick Douglass said that. Quoted in Harriet Beecher Stowe, *Men of Our Times* (Hartford, 1868), 395. Contemporary observers who noticed black work habits after emancipation rarely told of "laziness" but nearly always noticed irregularity, and in 1909 W. E. B. Du Bois quoted approvingly a writer who suggested that "what is termed Negro 'laziness' may be a means of making modern workingmen demand more rational rest and enjoyment rather than permitting themselves to be made machines." W. E. B. Du Bois, *Negro-American Family* (Atlanta, 1909), 42. See also Du Bois's discussion of the same matter in *World's Week*, 103 (1926), quoted in Asa H. Gordon, *Sketches of Negro Life and History in South Carolina* (Industrial College, Ga., 1929), 10–11.

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Mechanics who . . . did throw into Boston Harbor the Tea." In 1837 a crowd dumped flour, not tea, and in its behavior revealed a commonplace form of premodern protest, a complaint against what Thompson calls "the extortionate mechanisms of an unregulated market economy." The crowd in City Hall Park heard protests about the high price of rent, food, and especially flour and denunciations of "engrossers," and the New York *Herald* called the gathering "a flour meeting—a fuel meeting—a rent meeting—a food meeting—a bread meeting—every kind of a meeting except a political meeting." But a New York newspaper had printed advice from Portland, Maine, that "speculating" flour dealers be punished with "some mark of public infamy," and after the meeting adjourned a crowd (estimates range from two hundred to several thousand) paraded to Eli Hart's wholesale flour depot. A speaker advised it to "go to the flour stores and offer a fair price, and if refused take the flour." Crowd members dumped two hundred barrels of flour and one thousand bushels of wheat in the streets, broke windows, did other minor damage, and chased the city's mayor with stones and "balls of flour." At first, little looting occurred, and when wagons finally appeared to carry home sacks of flour "a tall athletic fellow in a carman's frock" shouted: "No plunder, no plunder; destroy as much as you please. Teach these monopolists that we know our rights and will have them, but d--n it don't rob them." The crowd moved on to other flour wholesalers and continued its work. It smashed the windows of B. S. Herrick and Son, dumped more flour, and finally stopped when "a person of respectable appearance" came from inside the building to promise that what remained untouched would be distributed gratis the next day to the "poor." The crowd cheered and melted away. More than twenty-eight persons were arrested (among them "mere boys," a few "black and ignorant laborers," a woman, and as yet unidentified white men), but the *Herald* found "mere humbug . . . the unholy cry of 'It's the foreigners who have done all this mischief'." The daily press, including the *Herald*, denounced the crowd as "the very canaille of the city," but the *Herald* also pleaded for the reimposition of the assize of bread. "Let the Mayor have the regulation of it," said the *Herald*. "Let the public authorities regulate the price of such an essential of life." (In 1857, incidentally, New Yorkers again filled the City Hall Park to again demand the restoration of the assize of bread and to ask for public works.)[42]

More than half a century later different New York City workers re-

[42] John R. Commons and others, eds., *Documentary History of American Industrial Society* (Cleveland, 1910), 5: 314–22; New York *Herald*, Feb. 13–16, 1837; New York *Evening Post*, Feb. 14, 16, 1837; New York *Sun*, n.d., quoted in Thomas Brothers, *United States of America as They Are* (London, 1840), 374–76; E. P. Thompson, "The Moral Economy of the English Crowd in the Eighteenth Century," *Past and Present*, 50 (1971): 76–136 but especially 134. On the Confederate bread riots, see Paul Angle and Earl S. Miers, eds., *Tragic Years, 1860–1865* (New York, 1960), 1: 526–28; William J. Kimball, "The Bread Riot in Richmond," *Civil War History*, 7 (1961): 149–54. Early American patterns of price regulation involving foodstuffs and the disputes over them are detailed splendidly in Richard B. Morris, *Government and Labor in Early America* (New York, 1946), *passim*, and Sam Bass Warner, *The Private City* (Philadelphia, 1968), ch. 1.

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enacted the 1837 food "riot." Unlike the rioters of 1837 in origins and rhetoric, the later rioters nevertheless displayed strikingly similar behavior. In 1902, and a few years before Upton Sinclair published *The Jungle,* orthodox New York City Jews, mostly women and led by a woman butcher, protested the rising price of kosher meat and the betrayal of a promised boycott of the Meat Trust by retail butchers. The complaint started on the Lower East Side and then spontaneously spread among Jews further uptown and even among Jews in Brooklyn, Newark, and Boston. The Lower East Side Jews demanded lower prices. Some called for a rabbi to fix for the entire New York Jewish community the price of meat, as in the East European *shtetl.* Others formed a cooperative retail outlet. But it is their behavior that reveals the most. The nation's financial metropolis saw angry immigrant women engage in seemingly archaic traditional protest. Outsiders could not understand its internal logic and order. These women did not loot. Like the 1837 demonstrators, they punished. Custom and tradition that reached far back in historical time gave a coherence to their rage. The disorders started on a Wednesday, stopped on Friday at sundown, and resumed the following evening. The women battered butcher shops but did not steal meat. Some carried pieces of meat "aloft on pointed sticks . . . like flags." Most poured kerosene on it in the streets or in other ways spoiled it. "Eat no meat while the Trust is taking meat from the bones of your women and children," said a Yiddish circular apparently decorated with a skull and crossbones. The New York police and the New York *Times* came down quite hard on these Jewish women. A "dangerous class . . . very ignorant," said the *Times,* explaining:

> They mostly speak a foreign language. They do not understand the duties or the rights of Americans. They have no inbred or acquired respect for law and order as the basis of the life of the society into which they have come. . . . The instant they take the law into their own hands . . . they should be handled in a way that they can understand and cannot forget. . . . Let the blows fall instantly and effectively.

Two days later, the *Times* reflected on a British Royal Commission then examining the effects of Jewish immigration on British society. "Stepney," the *Times* of New York noted, also was "becoming a foreign town. . . . Perhaps when the Royal Commission reports on what England should do about its un-English Londoners we shall learn what to do about these not yet Americanized New Yorkers whose meat riots were stranger than any nightmare." The *Times* found comfort in what it felt to be a "fact." Immigrant Jews had sparked the 1902 troubles. "The attempted incendiarism," it believed, "could not happen in an American crowd at all." The New York *Times* had done more than idealize a world that had never been lost in suggesting that premodern Americans had been little more than ordered and expectant entrepreneurs. In comparing its response in 1902

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to that of the New York *Herald* in 1837, we measure some of the distance that proper Americans had travelled from their own, premodern American roots.[43]

Even though American society itself underwent radical structural changes between 1815 and the First World War, the shifting composition of its wage-earning population meant that traditional customs, rituals, and beliefs repeatedly helped shape the behavior of its diverse working-class groups. The street battle in 1843 that followed Irish efforts to prevent New York City authorities from stopping pigs from running loose in the streets is but one example of the force of old styles of behavior. Both the form and the content of much expressive working-class behavior, including labor disputes, often revealed the powerful role of secular and religious rituals. In 1857 the New York City unemployed kidnapped a musical band to give legitimacy to its parade for public works. After the Civil War, a Fall River cotton manufacturer boasted that the arrival of fresh Lancashire operatives meant the coming of "a lot of greenhorns here," but an overseer advised him, "Yes, but you'll find they have brought their horns with them." A few years later, the Pittsburgh courts prevented three women married to coal miners from "tin-horning" nonstrikers. The women, however, purchased mouthorgans. ("Tin-horning," of course, was not merely an imported institution. In Franklin, Virginia, in 1867, for example, a Northern white clergyman who started a school for former slave children had two nighttime "tin horn serenade[s]" from hostile whites.) Recurrent street demonstrations in Paterson accompanying frequent strikes and lockouts nearly always involved horns, whistles, and even Irish "banshee" calls. These had a deep symbolic meaning, and, rooted in a shared culture, they sustained disputes. A Paterson manufacturer said of nonstrikers: "They cannot go anywhere without being molested or insulted, and no matter what they do they are met and blackguarded and taunted in a way that no one can stand . . . which is a great deal worse than actual assaults." Another manufacturer agreed:

> All the police in the world could not reach the annoyances that the weavers have at home and on the street that are not offenses—taunts and flings, insults and remarks. A weaver would rather have his head punched in than be called a "knobstick," and this is the class of injury they hate worst, and that keeps them out more than direct assault.

But the manufacturers could not convince the town's mayor (himself a British immigrant and an artisan who had become a small manufacturer) to ban street demonstrations. The manufacturers even financed their own

[43] New York *Herald,* Apr. 21, 23, May 15–30, 1902; New York *Tribune,* Apr. 19, 21, May 11, 16–27, June 15, 1902; New York *World,* May 16–19, 1902; New York *Commercial Advertiser,* May 15, 17, 24, 26, 1902; New York *Times,* May 23–26, June 7, 1902; New York *Journal,* May 15, 1902; *People* (N.Y.), May 14, 15, 20, 23, 26, 1902. Food riots occurred again among immigrant New York City Jews in the spring of 1917.

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private militia to manage further disorders, but the street demonstrations continued with varying effectiveness until 1901 when a court injunction essentially defined the streets as private space by banning talking and singing banshee (or death) wails in them during industrial disputes. In part, the frequent recourse to the courts and to the state militia after the Civil War during industrial disputes was the consequence of working-class rituals that helped sustain long and protracted conflicts.[44]

Symbolic secular and, especially, religious rituals and beliefs differed among Catholic and Jewish workers fresh to industrial America between 1894 and the First World War, but their function remained the same. Striking Jewish vestmakers finished a formal complaint by quoting the Law of Moses to prove that "our bosses who rob us and don't pay us regularly commit a sin and that the cause of our union is a just one." ("What do we come to America for?" these same men asked. "To bathe in tears and to see our wives and children rot in poverty?") An old Jewish ritual oath helped spark the shirtwaist strike of women workers in 1909 that laid the basis for the International Ladies' Garment Workers Union. A strike vote resulted in the plea, "Do you mean faith? Will you take the old Jewish oath?" The audience responded in Yiddish: "If I turn traitor to the cause, I now pledge, may this hand wither and drop off at the wrist from the arm I now raise." (Incidentally, during this same strike a magistrate who advised troublesome Jewish women that "you are on strike against God" provoked Bernard Shaw's classic quip, "Delightful, medieval America always in the most intimate personal confidence of the Almighty.") Immigrant Catholic workers shared similar experiences with these immigrant Jews. A reporter noticed in 1910 at a meeting of striking Slavic steel workers in Hammond, Indiana: "The lights of the hall were extinguished. A candle stuck into a bottle was placed on a platform. One by one the men came and kissed the ivory image on the cross, kneeling before it. They swore not to scab." Not all rituals were that pacific. That same year, Slavic miners in Avelia, Pennsylvania, a tiny patch on the West Virginia border, crucified George Rabish, a mine boss and an alleged labor spy. An amazed journalist felt their behavior "in the twentieth century . . . almost beyond belief":

> Rabish was dragged from his bed and driven out into the street amid the jeers of the merciless throng. . . . Several men set about fashioning a huge cross out of mine timbers. They even pressed a crown of thorns upon his temples. After they had nailed him to the cross, the final blasphemy was to dance and sing about the still living man.

[44] Billington, *Protestant Crusade,* 196; New York *Herald,* Nov. 12, 1857; Fall River *Weekly News,* Jan. 21, 1875; L. H., Pittsburgh, to the editor, *John Swinton's Paper* (New York), Sept. 28, 1884; A. B. Corliss, Franklin, Va., to the editor, *American Missionary,* 11 (1867): 27–28; Paterson *Press,* Aug. 2, 1877; Paterson *Guardian,* Aug. 2, 1877; Gutman, "Class, Status, and Community Power"; 283–87; Gutman, "Social Structure and Working-Class Life and Behavior in an Industrial City, Paterson, New Jersey, 1830–1905," unpublished manuscript.

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Fig. 9. Striker argues with a strikebreaker in New York City. Photograph by Lewis W. Hine. Courtesy George Eastman House Collection.

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That event was certainly unusual, but it was commonplace for time-honored religious symbols as well as American flags to be carried in the frequent parades of American workers. Western Pennsylvania Slavic and Italian coal miners in a bitter strike just east of Pittsburgh (eighteen of twenty thousand miners quit work for seventeen months when denied the right to join the United Mine Workers of America) in 1910 and 1911 carried such symbols. "These rural marches," said Paul Kellogg, "were in a way reminiscent of the old time agrarian uprisings which have marked English history." But theirs was the behavior of peasant and village Slavs and Italians fresh to modern industrial America, and it was just such tenacious peasant-worker protests that caused the head of the Pennsylvania State Police to say that he modeled his force on the Royal Irish Constabulary, not, he insisted, "as an anti-labor measure" but because "conditions in Pennsylvania resembled those in strife-torn Ireland." Peasant parades and rituals, religious oaths and food riots, and much else in the culture and behavior of early twentieth-century immigrant American factory workers were cultural anachronisms to this man and to others, including Theodore Roosevelt, William Jennings Bryan, Elbert Gary, and even Samuel Gompers, but participants found them natural and effective forms of self-assertion and self-protection.[45]

THE PERSPECTIVE emphasized in these pages tells about more than the behavior of diverse groups of American working men and women. It also suggests how larger, well-studied aspects of American society have been affected by a historical process that has "industrialized" different peoples over protracted periods of time. Fernand Braudel reminds us that "victorious events come about as the result of many possibilities," and that "for one possibility which actually is realized, innumerable others have drowned." Usually these others leave "little trace for the historian." "And yet," Braudel adds, "it is necessary to give them their place because the losing movements are forces which have at every moment affected the final outcome." Contact and conflict between diverse preindustrial cultures and a changing and increasingly bureaucratized industrial society also affected the larger society in ways that await systematic examination. Contemporaries realized this fact. Concerned in 1886 about the South's "dead"—that is, unproductive—population, the Richmond *Whig* felt the "true remedy" to be "educating the industrial morale of the people." The

[45] Rischin, *Promised City*, 144–94; Levine, *Women's Garment Workers*, 154; Graham Adams, *Age of Industrial Violence, 1910–1915* (New York, 1966), 105–16, 188–94; Chicago *Socialist*, Jan. 31, 1910, quoted in Brody, *Steelworkers in America*, 125–46; Cleveland *Plain Dealer*, Apr. 24, 1910 (courtesy of Robert D. Greenberg); Paul Kellogg and Shelby M. Harrison, "The Westmoreland Strike," *Survey*, 25 (1910), 345–66; *Report on the Miners' Strike in the Bituminous Coal Fields in Westmoreland County, Pennsylvania, in 1910–1911* (Washington, 1912), *passim*. A recent work which convincingly disputes earlier views that Slavic coal miners were difficult to organize into trade unions is Victor H. Greene, *Slavic Community on Strike* (Notre Dame, 1968).

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Whig emphasized socializing institutions primarily outside of the working class itself. "In the work of inculcating industrial ideas and impulses," said the *Whig*, "all proper agencies should be enlisted—family discipline, public school education, pulpit instruction, business standards and requirements, and the power and influence of the workingmen's associations." What the *Whig* worried over in 1886 concerned other Americans before and after that time. And the resultant tension shaped society in important ways. Some are briefly suggested here. In a New York *Times* symposium ("Is America by Nature a Violent Society?") soon after the murder of Martin Luther King, the anthropologist Clifford Geertz warned: "Vague references to the frontier tradition, to the unsettledness of American life, to our exploitative attitude toward nature or to our 'youthfulness' as a nation, provide us with prefabricated 'explanations' for events we, in fact, not only do not understand, but do not want to understand." More needs to be said than that Americans are "the spiritual descendants of Billy the Kid, John Brown, and Bonnie and Clyde." It has been suggested here that certain recurrent disorders and conflicts relate directly to the process that has continually "adjusted" men and women to regular work habits and to the discipline of factory labor. The British economic historian Sidney Pollard reminds us that this "task, different in kind" is "at once more subtle and more violent from that of maintaining discipline among a proletarian population of long standing."[46]

The same process has even greater implications for the larger national American culture. Hannah Arendt has brilliantly suggested that the continual absorption of distinctive native and foreign "alien" peoples has meant that "each time the law had to be confirmed anew against the lawlessness inherent in all uprooted people," and that the severity of that process helps explain to her why the United States has "never been a nation-state."[47] The same process also affected the shaping and reshaping of American police and domestic military institutions. We need only realize that the burning of a Boston convent in 1834 by a crowd of Charlestown truckmen and New Hampshire Scotch-Irish brickmakers caused the first revision of the Massachusetts Riot Act since Shays' Rebellion, and that three years later interference by native firemen in a Sunday Irish funeral procession led to a two-hour riot involving upwards of fifteen thousand persons (more than a sixth of Boston's population), brought militia to that city for the first time, and caused the first of many reorganizations of the Boston police force.[48] The regular contact between alien work

[46] Richmond *Whig*, June 15, 1886 (courtesy of Leon Fink); Clifford Geertz, "We Can Claim No Special Gift for Violence," New York *Times Magazine*, Apr. 28, 1968, pp. 24–25; Pollard, "Factory Discipline in the Industrial Revolution," 254–71.

[47] Hannah Arendt, "Lawlessness Is Inherent in the Uprooted," New York *Times Magazine*, Apr. 28, 1968, pp. 24–25.

[48] Oscar Handlin, *Boston's Immigrants* (New York, 1968), 186–91; Roger Lane, *Policing the City: Boston* (Cambridge, Mass., 1967), chs. 1–2.

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cultures and a larger industrializing or industrial society had other consequences. It often worried industrialists, causing C. E. Perkins, the president of the Chicago, Burlington, and Quincy Railroad to confide in a friend in the late nineteenth century, "If I were able, I would found a school for the study of political economy in order to harden men's hearts." It affected the popular culture. A guidebook for immigrant Jews in the 1890s advised how to make it in the New World: "Hold fast, this is most necessary in America. Forget your past, your customs, and your ideals. . . . A bit of advice to you: do not take a moment's rest. Run, do, work, and keep your own good in mind."[49] Cultures and customs, however, are not that easily discarded. So it may be that America's extraordinary techno-



Fig. 10. Jewish peddler in Chicago, ca. 1910. Photograph by Lewis W. Hine. Courtesy George Eastman House Collection.

logical supremacy—its talent before the Second World War for developing labor-saving machinery and simplifying complex mechanical processes—depended less on "Yankee know-how" than on the continued infusion of prefactory peoples into an increasingly industrialized society.[50] The same

[49] Sidney Fine, *Laissez Faire and the General Welfare State* (Ann Arbor, 1956), 54, 56, 103; Rischin, *Promised City*, 75.

[50] John Higham, in C. Vann Woodward, ed., *Comparative Approaches to American History* (New York, 1968), 101; H. J. Habakkuk, *American and British Technology in the Nineteenth Century* (Cambridge, 1967), *passim*.

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process, moreover, may also explain why movements to legislate morality and to alter habits have lasted much longer in the United States than in most other industrial countries, extending from the temperance crusades of the 1820s and the 1830s to the violent opposition among Germans to such rules in the 1850s and the 1860s and finally to formal prohibition earlier in this century.[51] Important relationships also exist between this process and the elite and popular nativist and racist social movements that have ebbed and flowed regularly from the 1840s until our own time, as well as between this process and elite political "reform" movements between 1850 and the First World War.[52]

The sweeping social process had yet another important consequence: it

[51] Although the literature on American temperance and prohibition movements is vast, nothing yet written about them approaches in clarity of analysis and use of evidence Brian Harrison, *Drink and the Victorians: The Temperance Question in England, 1815–1872* (Pittsburgh, 1971). Much information on the relationship between temperance and late nineteenth-century American factory labor is found in the little-used *U.S. Commissioner of Labor, Twelfth Annual Report, 1897* (Washington, 1897), a detailed analysis of the replies about working-class drinking habits from the owners of more than seven thousand establishments which together employed about 1,750,000 workers. For the later period see (but with great care), Herman Feldman, *Prohibition: Its Economic and Industrial Aspects* (New York, 1927), especially pages 200–12. Feldman, who surveyed representative manufacturing firms about the impact of Prohibition on work patterns, learned that "many plants in pre-Prohibition days had the five-day week long before Henry Ford ever thought of it, because so many workers were absent after pay-day." Employers used "considerable ingenuity" to cut down Monday absenteeism. Some had shifted pay-day from Saturday to a mid-week work day, and others paid wages less frequently. Feldman received replies from 287 firms. Two-thirds said improved attendance at work followed Prohibition. A New Hampshire shoe manufacturer no longer had to "reckon with the after-effects of celebrations, holidays, and weekends" as he did "years ago." And a St. Louis metal manufacturer told that the Saturday paycheck no longer meant "the usual 'Blue Monday.'" "Now," he explained, "we have changed to Friday, and as we are paying by the check system this enables the men to deposit their checks in one of the local banks that stay open on Friday evenings. We have no Saturday absences." Not all sounded so optimistic. "The stuff available to labor," said an employer of Delaware River tugboat and barge workers, "and there is plenty of it, is so rotten that it takes the drinking man two to three days to get over his spree." And a Connecticut manufacturer feared that new technology threatened regular attendance at work more than traditional or spurious spirits. "Cheap automobiles," he said, "make more employees tardy than does liquor."

[52] Detailed local studies are badly needed here, and these should focus on the clear continuities between antebellum municipal "reform" movements and the issues that dominated much of local politics in the Gilded Age. Such studies will reveal neglected elements of continuity in political issues, patterns of elite reform, and patterns of political centralization that started before the Civil War and continued into the Progressive Era. Few saw this more clearly than President Andrew D. White of Cornell University who reminded delegates to the First Lake Mohonk Conference on the Negro Question in 1890 that "in 1847" New York had "sank back toward mobocracy." "We elected judges on small salaries for short terms," said White; "we did the same thing with the governors. We have swung backward or forward . . . out of that. We now elect men for longer terms. In many ways, we have returned to more conservative principles." Isabel Barrows, ed., *First Lake Mohonk Conference on the Negro Question* (Boston, 1890), 120. See also Samuel P. Hays, "The Politics of Reform in Municipal Government in the Progressive Era," *Pacific Northwest Quarterly*, 55 (1964): 157–69. The pattern Hays uncovered for Progressive Pittsburgh was not new because its roots rested in elite fears of immigrant and working-class domination of municipal governments (and especially the influence of those groups on local fiscal and educational policies), fears that revealed themselves powerfully before the Civil War and retained much importance during the Gilded Age. The focus on municipal corruption has hidden such important social and political processes from historians. See the original and convincing study by Douglas V. Shaw, "The Making of an Immigrant City: Ethnic and Cultural Conflict in Jersey City, New Jersey, 1850–1877" (Ph.D. dissertation, University of Rochester, 1972), that demonstrates conclusively (for that city at least) that antebellum elite nativism did not end with the Civil War but continued into the postwar decades.

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reinforced the biases that otherwise distort the ways in which elite observers perceive the world below them. When in 1902 the New York *Times* cast scorn upon and urged that force be used against the Jewish women food rioters, it conformed to a fairly settled elite tradition. Immigrant groups and the working population had changed in composition over time, but the rhetoric of influential nineteenth- and early twentieth-century elite observers remained constant. Disorders among the Jersey City Irish seeking wages due them from the Erie Railroad in 1859 led the Jersey City *American Standard* to call them "imported *beggars*" and "*animals*", "a mongrel mass of ignorance and crime and superstition, as utterly unfit for its duties, as they are for the common courtesies and decencies of civilized life." (According to their historian Earl Niehaus, the antebellum New Orleans Irish fared so bady in the "public" view that many non-Irish criminals, Germans and even blacks among them, assumed Irish names.) Although the Civil War ended slavery, it did not abolish these distorted perceptions and fears of new American workers. In 1869 *Scientific American* welcomed the "ruder" laborers of Europe but urged them to "assimilate" quickly or face "a quiet but sure extermination." Those who retained their alien ways, it insisted, "will share the fate of the native Indian." Elite nativism neither died out during the Civil War nor awaited a rebirth under the auspices of the American Protective Association and the Immigration Restriction League. In the mid-1870s, for example, the Chicago *Tribune* called striking immigrant brickmakers men but "not reasoning creatures," and the Chicago *Post-Mail* described that city's Bohemian residents as "depraved beasts, harpies, decayed physically and spiritually, mentally and morally, thievish and licentious." The *Democratic Chicago Times* cast an even wider net in complaining that the country had become "the cess-pool of Europe under the pretense that it is the asylum of the poor." Most Chicago inhabitants in the Gilded Age were foreign-born or the children of the foreign-born, and most English-language Chicago newspapers scorned them. The Chicago *Times* told readers that Slavic Chicagoans were descended from "the Scythians," "eaters of raw animal food, fond of drinking the blood of their enemies whom they slew in battle, and [men] who preserved as trophies the scalps and skins of enemies whom they overthrew." "The old taste for the blood of an enemy has never been obliterated," said this proper Chicago newspaper. And the Slavs had now "invaded the peaceful republic." In words echoed differently in the New York *Times* fifteen years later, the Chicago *Times* advised: "Let us whip these slavic wolves back to the European dens from which they issue, or in some way exterminate them." Here, as in the Jersey City *American Standard* (1859) and the New York *Times* (1902), much more was involved than mere ethnic distaste or "nativism." In quite a different connection and in a relatively homogeneous country, the Italian Antonio Gramsci concluded of such evidence that "for a social

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elite the features of subordinate groups always display something barbaric and pathological." The changing composition of the American working class may make so severe a dictum more pertinent to the United States than to Italy. Class and ethnic fears and biases combined together to worry elite observers about the diverse worlds below them and to distort gravely their perceptions of these worlds. Few revealed these perceptual difficulties and genuine fears more clearly than John L. Hart in 1879:

> About one half of our poor can neither read nor write, have never been in any school, and know little, positively nothing, of the doctrines of the Christian religion, or of moral duties, or of any higher pleasures than beer-drinking and spirit-drinking, and the grossest sensual indulgence. . . . They have unclear, indefinable ideas of all around them; they eat, drink, breed, work, and die; and while they pass through their brute-like existence here, the rich and more intelligent classes are obliged to guard them with police and standing armies, and to cover the land with prisons, cages, and all kinds of receptacles for the perpetrators of crime.

Hart was not an uneducated "nativist." He had been professor of rhetoric, the English language, and literature at the College of New Jersey and also the principal of the New Jersey State Normal School. These words appeared in his book entitled *In the School-Room* (1879) where he argued that "schoolhouses are cheaper than jails" and that "teachers and books are better security than handcuffs and policemen." We have returned to Lesson One.[53]

THESE PAGES have fractured historical time, ranging forward and backward, to make comparisons for several reasons. One has been to suggest how much remains to be learned about the transition of native and foreign-born American men and women to industrial society, and how that transition affected such persons and the society into which they entered. "Much of what gets into American literature," Ralph Ellison has shrewdly observed, "gets there because so much is left out." That has also been the case in the writing of American working-class history, and the framework and methods suggested here merely hint at what will be known about American workers and American society when the many transitions are

[53] Jersey City *American Standard,* Sept. 20, 1859 (courtesy of Douglas V. Shaw); Earl Niehaus, *Irish in New Orleans* (Baton Rouge, 1965), 186; *Scientific American,* June 19, 1869, pp. 393–94; Chicago *Tribune,* May 11, 1876; Chicago *Post and Mail,* n.d., reprinted in Chicago *Tribune,* July 25, 1876; Chicago *Times,* Apr. 25, 1874; Chicago *Times,* May 6, 1886 (courtesy of Steven Hahn); Antonio Gramsci, quoted in Charles Tilly, "Collective Violence in European Perspective," in Hugh D. Graham and Ted R. Gurr, eds., *Violence in America* (New York, 1969), 12; John L. Hart, *In The School-Room* (Philadelphia, 1879), 252–57 (courtesy of Barbara Berman). See also John Kober, *Capone, The Life and World of Al Capone* (New York, 1972), 344, for an extraordinary description of Alcatraz prison routine in the 1930s: "Midmorning. Bell. Recess. Bell. Work. 11:30. Bell. Prisoners Counted. Bell. Noon. Bell. Lunch. 1 P.M. Bell. Work. Midafternoon. Bell. Recess. Work. 4:30. Bell. Prisoners Counted. Bell. 6:30. Bell. Lockup. 9:30. Bell. Lights Out."

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studied in detail. Such studies, however, need to focus on the particularities of both the group involved and the society into which they enter. Transitions differ and depend upon the interaction between the two at specific historical moments. But at all times there is a resultant tension. Thompson writes:

> There has never been any single type of "the transition." The stress of the transition falls upon the whole culture: resistance to change and assent to change arise from the whole culture. And this culture includes the systems of power, property-relations, religious institutions, etc., inattention to which merely flattens phenomena and trivializes analysis.

Enough has been savored in these pages to suggest the particular importance of these transitions in American social history. And their recurrence in different periods of time indicates why there has been so much discontinuity in American labor and social history. The changing composition of the working population, the continued entry into the United States of nonindustrial people with distinctive cultures, and the changing structure of American society have combined together to produce common modes of thought and patterns of behavior. But these have been experiences disconnected in time and shared by quite distinctive first-generation native and immigrant industrial Americans. It was not possible for the grandchildren of the Lowell mill girls to understand that their Massachusetts literary ancestors shared a great deal with their contemporaries, the peasant Slavs in the Pennsylvania steel mills and coal fields. And the grandchildren of New York City Jewish garment workers see little connection between black ghetto unrest in the 1960s and the Kosher meat riots seventy years ago. A half century has passed since Robert Park and Herbert Miller published W. I. Thomas's *Old World Traits Transplanted,* a study which worried that the function of Americanization was the "destruction of memories."[54]

Not all fled such a past. Born of Croatian parents in McKeesport, Pennsylvania, in 1912 (his father and brother later killed in industrial accidents), Gabro Karabin published a prize-winning short story in *Scribner's Magazine* (1947) that reflected on the experiences replayed in different ways by diverse Americans and near-Americans:

> Around Pittsburgh, a Croat is commonplace and at no time distinctive. As people think of us, we are cultureless, creedless, and colorless in life, though in reality we possess a positive and almost excessive amount of those qualities. Among ourselves, it is known that we keep our culture to ourselves because of the heterogeneous and unwholesome grain of that about us. . . . We are, in the light of general impression, just another type of laboring foreigner . . . fit only as industrial fuel.

[54] Ralph Ellison and James Alan McPherson, "Indivisible Man," *Atlantic,* 226 (1970): 57; Thompson, "Time, Work-Discipline, and Industrial Capitalism," 80; Park and Miller, *Old World Traits Revisited,* 281. I am indebted to Leon Stein, the editor of *Justice,* for calling to my attention the fact that W. I. Thomas, whose great study of the Polish immigrant leaves us all in his debt, was the author of *Old World Traits Revisited.*

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The native-born American poet William Carlos Williams made a similar point. He lived near the city of Paterson and grasped its tragic but rich and deeply human interior textures far more incisively than temporary visitors such as Alexander Hamilton and William D. Haywood and illustrious native sons such as William Graham Sumner and Nicholas Murray Butler. The poet celebrated what gave life to a city in which men, women, and children made iron bars and locomotives and cotton and silk cloth:

> It's the anarchy of poverty
> delights me, the old
> yellow wooden house indented
> among the new brick tenements
>
> Or a cast iron balcony
> with panels showing oak branches
> in full leaf. It fits
> the dress of the children
>
> reflecting every stage and
> custom of necessity—
> chimneys, roofs, fences of
> wood and metal in an unfenced
> age and enclosing next to
> nothing at all: the old man
> in a sweater and soft black
> hat who sweeps the sidewalk—
>
> his own ten feet of it—
> in a wind that fitfully
> turning his corner had
> overwhelmed the entire city.

Karabin and Carlos Williams interpreted life and labor differently from the Chicago *Times* editor who in the Centennial Year (1876) boasted that Americans did not enquire "when looking at a piece of lace whether the woman who wove it is a saint or a courtesan."[55]

[55] Gabro Karabin, quoted in George J. Prpic, *Croatian Immigrants in America* (New York, 1971), 331–32; William Carlos Williams, "The Poor," in Louis Untermyer, ed., *Modern American and Modern British Poetry* (rev. ed.; New York, 1955), 132; Chicago *Times*, May 22, 1876.

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APPENDIX

TABLE 3. MALE OCCUPATIONAL STRUCTURE AND HOUSEHOLD COMPOSITION, SELECTED JEWS AND ITALIANS, NEW YORK CITY, 1905

	Jews	*Italians*
Total Males 20 and Older	6250	4518
Total Females 20 and Older	4875	3433
Male Occupational Structure		
Unskilled Laborer	7.7%	39.1%
Clothing Worker	44.7%	18.0%
Skilled Worker (Nonclothing)	21.5%	29.2%
Nonlaborer	26.1%	13.7%
Household Composition		
Percentage of All Households with a Nuclear Kin-related Core	96.6%	94.5%
Number of Kin-related Households	3584	2945
Number of Subfamilies	159	262
Nuclear Households	48.6%	59.9%
Extended Households	11.8%	23.2%
Augmented Households	43.1%	21.1%
Percentage of Households and Subfamilies with a Husband and/or Father Present	93.2%	92.9%

Note: As in 1880 the percentages again total more than 100 per cent because a small number of households that included both lodgers and relatives are counted twice.

The data are drawn from the New York State 1905 manuscript census schedules, and I am indebted to Mark Sosower, Leslie Neustadt, and Richard Mendales for gathering this material. As with the 1880 Paterson data, they cast grave doubts on the widely held belief that working-class family disruption commonly occurred as the by-product of immigration, urbanization, and factory work. The 1905 Jews studied lived on the Lower East Side (Rutgers, Cherry, Pelham, Monroe, Water, Pike, Jefferson, Clinton, Madison, Livingston, Henry, Division, Montgomery, Delancey, Rivington, Norfolk, Suffolk, and East Third Streets, East Broadway, and Avenue B). The Italians resided on Hancock, Thompson, Mulberry, Bayard, Mott, Canal, Baxter, Elizabeth, Spring, Prince, Grand, Hester, McDougal, Sullivan, West Houston, Bleecker, Bedford, Downing, and Carmine Streets, and the Bowery. The table above deserves another brief comment. Clothing workers are listed as a separate occupational category because census job descriptions make it impossible to determine their skill levels. A large percentage of those listed as nonlaborers engaged in petty enterprise (including peddling): 10.9 per cent of all the Jewish males and 8.3 per cent of all the Italian males. On early twentieth-century immigrant households and family behavior, see Virginia Yans McLaughlin, "Patterns of Work and Family Organization Among Buffalo's Italians," *Journal of Interdisciplinary History*, 2 (1971): 299–314, and McLaughlin, "Like the Fingers of the Hand: The Family and Community Life of First-Generation Italian-Americans in Buffalo, New York" (Ph.D. dissertation, State University of New York, Buffalo, 1970).

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C. Vann Woodward and the Burden of Southern Liberalism

MICHAEL O'BRIEN

IF HISTORIANS of the American South prove as devoted to their *penates* as those of the American West, a considerable devotion ought to be laid before the reputation of C. Vann Woodward. Just as Frederick Jackson Turner pre-empted a generation of scholarship, Woodward is the only Southern historian of recent years to have come close to fashioning the discipline in his own image. His version of the Compromise of 1877, his conception of the Redeemer regimes, his account of the New South movement, Populism, and the rise of segregation have become the conventional wisdom. New study habitually takes his work on the postbellum South as the starting point. Historians may move to agree or disagree, but it is a tribute to his stature that one must begin with Woodward if the effort is to ring true. He has become the kind of figure about whom Richard Hofstadter once wrote: "If we are to have any new thoughts, if we are to have an intellectual identity of our own, we must make the effort to distinguish ourselves from those who preceded us, and perhaps pre-eminently from those to whom we once had the greatest indebtedness."[1]

The factual issues raised by Woodward's books have already generated much commentary. Woodward himself, as a phenomenon in the intellectual history of the South, is also of some interest.[2] He has, after all, fashioned the most compelling image of the American South offered by his generation.

I would like to thank Dr. Jonathan Steinberg for his invitation to speak to the Trinity Hall History Society and for the encouragement that led me to convert a casual essay into something a little more permanent.

[1] Richard Hofstadter, *The Progressive Historians: Turner, Beard, Parrington* (New York, 1968), xiv.

[2] See, for example, Charles E. Wynes, *Race Relations in Virginia, 1870–1902* (Charlottesville, 1961); Frenise A. Logan, *The Negro in North Carolina, 1876–1894* (Chapel Hill, 1964); and Paul M. Gaston, *The New South Creed: A Study in Southern Mythmaking* (New York, 1970). The only extended discussion of Woodward as an intellectual is by David Potter, "C. Vann Woodward," in Robin W. Winks and Marcus Cunliffe, eds., *Pastmasters: Some Essays on American Historians* (New York, 1969). See also Sheldon Hackney, *"Origins of the New South* in Retrospect," *Journal of Southern History*, 38 (1972): 191–216. I have a feeling that Mr. Hackney has exaggerated the conceptual triumph of Woodward among Southern historians by underestimating the crucial nature of the "Populism" of Woodward's perception.

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And his idea of the South has a strategy, not entirely born of the documents that have passed through his hands, that warrants discussion.

By his own confession, Woodward is a "liberal, even more, a southern white liberal."[3] If one adds the word "intellectual" to that phrase, one has a basic text for understanding him. Yet "intellectual" is a slippery term. Its meaning has changed since the emergence of a professional intelligentsia in the South at the turn of the century. As part of Woodward's importance has been to redefine that species, it would be useful briefly to examine what went before him.

THE LIFE OF a Southern intellectual in 1900 was not a comfortable one, especially if he was a liberal. He could expect to be regarded with suspicion by the society around him. William P. Trent, founder of the *Sewanee Review*, despondently complained in 1898 of

> the fact that at Sewanee what harasses me is not so much lack of books and of city life as it is the fact that a considerable portion of the people around me consider me a traitor and don't like me and only keep quiet because they are afraid of me.[4]

The Southern intellectual could have little anticipation of political influence. An occasional state governor—notably Charles Aycock of North Carolina—might listen to a few intellectuals, but the case was rare. So singular was Aycock that he was repaid with an extravagant devotion from those academics whom he enlisted in his cause. Edwin Alderman, president of the University of Virginia, described him as "the Lord Chatham of a reawakened American state."[5] But even Aycock had little faith in the intellect or the scholar as a guide to the realities of politics. He wrote to Edwin Mims, a friend of John Spencer Bassett, after the affair in 1903 when Bassett nearly lost his job at Trinity College for injudicious remarks about the greatness of Booker T. Washington: "Bassett wrote unwisely, untruly; his view is academic. He breathes the atmosphere of the cloister. He does not know men."[6]

The economic and intellectual position of the Southern liberal rested on one basic premise, an alliance with the Northeast. The South had no money and no inclination to sustain an intellectual elite. The Southern liberal was forced to seek help wherever he could. Southern philanthropists were few and far between, though the Duke family did help Trinity

[3] Potter, "Woodward," 475.

[4] William P. Trent to H. B. Adams, Jan. 8, 1898, in W. Stull Holt, ed., *Historical Scholarship in the United States, 1876–1901, As Revealed in the Correspondence of Herbert B. Adams* (Baltimore, 1938), 249.

[5] Alderman, "Charles Brantley Aycock: Epic Builder of Education," in Howard W. Odum, ed., *Southern Pioneers in Social Interpretation* (Chapel Hill, 1925), 81.

[6] Charles Aycock to Edwin Mims, Nov. 6, 1903, Edwin Mims Papers, Joint University Library, Nashville. On the Bassett affair, see Richard Hofstadter and Walter P. Metzger, *The Development of Academic Freedom in the United States* (New York, 1955), 445–50.

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College in North Carolina. Northerners did have money and, if persuaded, the inclination to "do something" for the backward region and, not incidentally, for the struggling Southern intelligentsia. The General Education Board, for example, was a child of Northern finance. Involved in stimulating the board's activities, liberals gained from helping to administer its largess. But their power was vicarious.[7]

The great magazines and publishing houses where the Southern author had to seek a market were in the North. Even when a book was issued it was mostly Northerners who read it, for Southerners were not great consumers of literature. Walter Hines Page bewailed the fact that "the southern people don't buy magazines or books. . . . They have no intellectual curiosity."[8] This matter of audience closely influenced the content of Southern liberal thought. But it perhaps only reinforced a natural tendency among these nationalists to find an intellectual *modus vivendi* with the North. New England, in the words of George W. Cable, was "the intellectual treasury of the United States,"[9] and Southern liberals drew freely on the capital accumulated there before the Civil War. Emerson became a name that could be mentioned in a North Carolina lecture and draw applause; Henry Grady found kind words for Lincoln himself.[10] The creed of the New South had its precedent in the history of New England, whose superiority seemed to be demonstrated by the verdict of the Civil War. Adding a version of racial adjustment, liberals in the South tried to create a society of which Horace Mann and William Gladstone might have been proud.[11]

The historians of this generation wrote in a manner congenial to their contemporary social beliefs. They plotted a distinctive graph of the Southern past. In 1800, they contended, the South dominated the counsels of American liberalism. Virginians like Thomas Jefferson and James Madison helped to define the democratic life of the new nation. With the passing of that generation the blight of slavery engendered a decline in the standard of Southern statesmanship. As Virginius Dabney insisted: "During the three decades immediately preceding the Civil War, openly-avowed liberalism was virtually extinct below the Mason-Dixon Line." William Dodd, too, saw a steady recession from the enlightenment of Monticello to the reactionary folly of Calhoun and the secessionist folly of Jefferson Davis.[12]

[7] For a succinct definition of Southern liberalism, see Gunnar Myrdal *et al., An American Dilemma: The Negro Problem and Modern Democracy* (New York, 1944); and Louis R. Harlan, *Separate and Unequal: Public School Campaigns and Racism in the Southern Seaboard States, 1901–15* (Chapel Hill, 1958).

[8] Walter Hines Page to Edwin Mims, Oct. 6, 1911, Mims Papers.

[9] Louis D. Rubin, Jr., *George W. Cable: The Life and Times of a Southern Heretic* (New York, 1969), 154.

[10] Gaston, *The New South Creed*, 88.

[11] That generation of Southern liberals was heavily Anglophile. In this Woodrow Wilson was not alone. Alderman not only compared Aycock to Chatham, he added a parallel with Gladstone.

[12] Virginius Dabney, *Liberalism in the South* (Chapel Hill, 1932), 415–16; Dodd, *Statesmen of the Old South; or, From Radicalism to Conservative Revolt* (New York, 1911).

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The war was the nadir of Southern history. Woodrow Wilson found more justice in the North's case than in the South's.[13] Admittedly he was unusual in the rigorousness with which he pursued the logic of his position; most preferred silence on the political righteousness of the war. As Southerners they could scarcely pour unbridled scorn upon the South's central experience, the Confederacy. They could, however, turn from the politics of the war to the battlefield. The war may have been wrongheaded, but it was bravely done. Robert E. Lee, in particular, benefited from this: the Lee who hesitated before casting his lot with the Confederacy, who comported himself with dignity in a desperate struggle and then set a model of gentle reconciliation after the war. He became a Southern saint, not just to liberals who celebrated the manner of his life but to conservatives who knew the fact of his Confederate allegiance.[14] The greatest monument to this phase of Southern liberal history—almost an annex to the history of Virginia—was the work of Douglas Southall Freeman, the Virginia newspaper editor. He produced four volumes on Lee, three volumes on Lee's lieutenants, and six volumes on George Washington, with a sense of genealogy that was impeccable.

These liberals did not believe in racial equality, so Reconstruction was regarded with little enthusiasm. They were, however, energetic nationalists. Their special moment in Southern history, after the wasteland between Jefferson and the Compromise of 1877, came with the New South.[15] They were its immediate heirs, sometimes its instigators. In John Spencer Bassett's eyes the period after 1877 was the hour of the middle class, which supplanted an economically and intellectually decadent planter class: "The rise of the middle class has been the most notable thing connected with the white population of the South since the War," he wrote in 1903. In 1926 his former colleague, Edwin Mims, published *The Advancing South,* a book that celebrated the history of that achievement, the arrival of the Southern bourgeoisie.[16]

One phase of the South's history was out of bounds: Populism. Although an account of social reform in his lifetime, Mims's book totally ignored the farmers. Virginius Dabney did mention them and even conceded them some influence on the course of Southern liberalism, but he referred to them with a typical condescension. About South Carolina, he explained:

[13] Woodrow Wilson, "States Rights (1850-60)," in *The Cambridge Modern History,* 7, ed. A. W. Ward (New York, 1903-12): 442.

[14] See, for example, Woodrow Wilson, *Robert E. Lee: An Interpretation* (Chapel Hill, 1924); Edwin Mims, "General Lee's Place in History," *Outlook,* 84 (1906): 978-82; and William P. Trent, *Robert E. Lee* (Boston, 1899).

[15] The exception is William Dodd, who took the agrarianism of the Jeffersonian creed more seriously than most and had grave doubts about industrialization. See Robert Dallek, *Democrat and Diplomat: The Life of William E. Dodd* (New York, 1968), 54-69.

[16] John Spencer Bassett, "The Industrial Decay of the Southern Planter," *South Atlantic Quarterly,* 2 (1903): 112; Edwin Mims, *The Advancing South: Stories of Progress and Reaction* (New York, 1926).

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The campaign of 1890 sounded the political knell of the lowland coterie which had been the controlling factor in South Carolina for centuries. The men who had carved for the Palmetto State a place of conspicuous prestige in the annals of the republic and had guided its destinies since its establishment as a British colony, were rudely pushed aside to make way for a raucous band of back-country farmers.[17]

This distaste was clearly the horror of genteel reformers for a blunt, often illiterate, and usually disrespectful company of democrats. It was a repugnance not diminished by the knowledge that these plebs often had more political power than their social betters. On the way to his wedding in 1898, Edwin Mims sat on a train in Arkansas and observed a young married couple: "It is interesting to watch them. They are ignorant, uncultured looking people, and marriage can mean but little to them. What a low conception of life they have! And so it is with marriage as with all other good things in the world."[18]

THIS WAS THE burden of Southern liberal history, when the young Comer Vann Woodward began his career as an intellectual in the 1930s. He started in two places, Atlanta and Chapel Hill, where the precarious condition of the Southern intellectual had been significantly improved. At Emory University and in and around Atlanta, he moved in circles that included Will Alexander, director of the Southern Interracial Commission. Alexander had been in the forefront of white liberal efforts to ease racial tension, which was a great restraint upon critical freedom.[19] Here Woodward broke some unusual ground for a Southern liberal by getting involved with both a black and a Communist. In 1932 Angelo Herndon, an organizer of unemployed blacks, was charged with the capital offense of inciting "insurrection." It was a trumped-up affair: the statute dated from the Civil War. Woodward became the temporary chairman of a local committee for Herndon's defense. But the case ended unhappily for the young historian when the Communist party took over the matter "as a propaganda device."[20] Here it is enough to note that Woodward showed a willingness to step outside the conventional wisdom and that he survived. He was not to escape completely unscathed, for Will Alexander was none too pleased about it all. Still Woodward went off to Chapel Hill in 1934 with a General Education Board fellowship in his pocket. With the possible exception of

[17] Dabney, *Liberalism in the South*, 206.

[18] Edwin Mims to Clara Puryear, June 18, 1898, Mims Papers. The best secondary sources for Southern historiography are Wendell Holmes Stephenson, *The South Lives in History* (Baton Rouge, 1955) and his *Southern History in the Making: Pioneer Historians of the South* (Baton Rouge, 1964), although Stephenson is sometimes a little overzealous in his addresses to the household gods. Some of his essays deal with conservative historians, like Ulrich Phillips, who are not directly relevant here.

[19] Wilma Dykeman and James Stokely, *Seeds of Southern Change: The Life of Will Alexander* (Chicago, 1962).

[20] Potter, "Woodward," 376–77.

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Nashville, Chapel Hill was the intellectual center of the South; it was unquestionably the hub of its liberal intelligentsia.

The University of North Carolina in the 1930s had not only a dedication to liberalism but also more money than in the days of Edwin Alderman. Between 1919 and 1930 the university's annual support rose from $270,097 to $1,342,774 and the size of its faculty went from 78 to 225.[21] Alderman had moaned in 1904, "I am so tired of making bricks without straw. I am so weary of trying to carry out ideas without means."[22] In 1901 the "total available income for the sixty-six colleges and universities of Virginia, North Carolina, South Carolina, Georgia, Alabama, Mississippi and Arkansas was $65,843 less than that of Harvard."[23] Efforts to provide the bricks and mortar of the Southern academy—reasonably good universities, adequate libraries, scholarships, good salaries—had begun to bear fruit when Woodward came to North Carolina. The edifice was still far from overwhelming, nor was it satisfactory to its inhabitants, but there were enough bricks for a respectable building.

The improved economics of Southern education had an important bearing on Woodward, the young liberal. In 1900 the search for the raw materials of an intellectual elite had deeply informed the character of Southern liberalism. That quest drove Southern liberals into dependence on the North, forcing them to fashion a mind congenial to that alliance. Although that dependence was still important in the 1930s, its very success had diminished its compulsive hold. The New Deal had interposed a new factor in the equation; Southern states had earlier begun to be more generous with appropriations; the reading public in the South had grown. The new intellectual could define himself with less reference to the old problems. A historian could look to phases of the Southern past whose lessons were not necessarily comfortable for the North Carolina-Virginia-Northeast axis.[24] He was, however, close enough to the days of Alderman to know that economic issues were important.

The priority of material problems was unlikely to be lost upon a Southerner during the Depression. Woodward showed his recognition of it by his enthusiasm for the work of Charles Beard. Beard's view of the Civil War as a struggle between nascent industrialism and agrarianism was quickly adopted by Woodward, who remained faithful to the vision.[25] Much later he was to write of his own *Reunion and Reaction:*

21 George B. Tindall, *The Emergence of the New South, 1913–1945* (Baton Rouge, 1967), 266. There was, however, retrenchment during the Depression.

22 Edwin Alderman to Walter Hines Page, Apr. 4, 1904, quoted in C. Vann Woodward, *Origins of the New South, 1877–1913* (Baton Rouge, 1951), 436.

23 Woodward, *Origins*, 437.

24 There is something to be said for the contention that the recent intellectual history of the South—especially in its liberal aspects—is a chapter in the intellectual imperialism of Chapel Hill. To this thesis Woodward would form no exception, though he added an Arkansan corollary.

25 There are also occasional echoes of Parrington. See Woodward, *Tom Watson: Agrarian Rebel* (New York, 1938); for instance: "the South also had its 'Great Barbecue'" (p. 52).

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In looking back over the citations . . . I was struck by the omission of one name—that of the late Charles A. Beard. It was, perhaps, an unconscious tribute that it did not seem necessary to mention him as the originator of the concept of the Civil War and Reconstruction as a revolution. . . . Yet my book is built upon that conception.[26]

Woodward, however, domesticated the Beardian viewpoint by seeing the strife as not merely sectional but also as between farm and factory within the South itself, a microcosm of the greater conflict.[27]

The Depression bred a skepticism about the promises of the New South. This alone put Woodward at a distance from the traditional schema of liberal historiography, since it precluded seeing the prevailing order as a fitting consummation to Southern history. But he shared his forebears' nationalism, so that many of the years between Jefferson and Appomattox were closed to him as to them. At the same time he tried to be free from their belief in racial inequality. The problem posed by these perceptions was severe. He solved it in the only logical way: he turned to Populism.[28]

He had shed the horror of the great unwashed that had deterred his predecessors from the Populist tradition. In his first book, a biography of the Populist Tom Watson, Woodward grasped at an aspect of the Southern liberal tradition that saw economic issues as a priority above nationalism and racialism. From the standpoint of 1955 Woodward was to observe:

It was easier in the 1930's than it is in the 1950's to understand the 1890's. One had then only to look about him. For the look of things in the South in the trough of the Great Depression did a lot toward making the desperate mood and temper of the South of the nineties wholly credible.[29]

In Watson, Woodward found a man who resisted the divisiveness of racialism and sought to yoke together poor white and black in a common front against economic oppression. He may have been lucky in this. Watson was not entirely typical in his sympathy for the black.[30] As Woodward himself was to establish, poor-white democracy went hand in hand with the elimination of the Negro from politics and the rise of Jim Crow. But there was a difficulty on this score even in Watson's career. The tribune of

[26] Woodward, *Reunion and Reaction* (2d rev. ed.; New York, 1956), v.

[27] This was a small part of Woodward's wider perception of the South as sharing in the broad currents of American life and so offering a home to the liberal as well as the conservative: a prospect otherwise destroyed if the South is identified solely with agrarianism.

[28] Woodward's new stance on race opened the possibility of Reconstruction as a fruitful area for the liberal historian. But he was not to pursue this until the 1950s. See Woodward, "Equality: The Deferred Commitment," *American Scholar*, 27 (1958): 459–72. In reconsidering Populism, Woodward was not alone. The 1930s saw a considerable revival of interest in that movement. See Arthur S. Link and Rembert Patrick, eds., *Writing Southern History: Essays in Historiography in Honor of Fletcher M. Green* (Baton Rouge, 1965), 366–68.

[29] Woodward, "Preface to the 1955 Reissue," *Tom Watson: Agrarian Rebel* (New York, 1955).

[30] See Robert Saunders, "Southern Populists and the Negro, 1893–1895," *Journal of Negro History*, 54 (1969): 240–61. This expresses reservations about the "Populist failure to accept the Negro as an intellectual, moral or political equal."

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the underdog in the early 1890s, Watson became in the twentieth century the foremost vilifier of Negro, Jew, and Catholic in Georgia. Woodward faithfully chronicled both aspects of Watson's life. He gave his enthusiasm to the former and his sorrowing regret to the latter. But Woodward insisted on separating the two halves of Watson's story, thereby neatly avoiding a considerable difficulty for himself as a Southern liberal. If the bitterness of Tom Watson was the logical and necessary consequence of his disappointed crusade, the moral was an uncomfortable one. By splitting the first part of Watson's career off from the second, Woodward neutralized the issue. He did admit that the two might be related but not that they were an organic whole. For it was crucial to Woodward the liberal to believe that Watson could have succeeded.

Woodward has consistently stressed change in the Southern past, because, as a liberal, he has wanted change. If Southern history is a remorseless unity, unbroken in its social conservatism and unchallenged by a significant radicalism, the task of the Southern liberal is futile. He is trying to sow on ground so stony that it is not worth buying the seed: one might as well go off to a better plot, as did many liberals of Trent's day. On the other hand, if Southern history is ferment and change, in which conservatism has only triumphed after a desperate struggle and holds an insecure victory, there is hope. It has been Woodward's insight to point to this volatility. In an analysis of Wilbur J. Cash, Woodward charged as a fault of Cash that he overestimated the unbroken flow of Southern history: "The history of the South . . . has always seemed to me characterized by discontinuity, and I have suggested this as one trait that helps account for the distinctiveness of the South and its history. . . . Southerners, unlike other Americans, repeatedly felt the solid ground of continuity give way under their feet."[31]

Woodward sought out those "forgotten alternatives"[32] that would have created a liberal South if they had succeeded. His *The Strange Career of Jim Crow* is the most sustained example of this search. There he examined the history of Southern race relations and noted that segregation, the Jim Crow of the statute book, was not the immediate sequel of emancipation. Dating from the 1890s, it was an institution scarcely old enough to encompass the lifespan of a single individual: it was not the immutable system that Southerners imagined in the passions of the 1950s. There was no golden age, but there was a moment of some gentleness in the relationship of black and white in the South. Now that the crisis over the dismantling of segregation has passed and with it the need to prove the transience of Jim Crow, the book reads less compellingly. It looks the most wistful of Woodward's attempts to find a usable past. As he himself has been forced to recognize, segregation was only the structure of a relationship and not

[31] Woodward, "W. J. Cash Reconsidered," *New York Review of Books*, Dec. 4, 1969, p. 32.

[32] This is the chapter heading of the first lecture in Woodward's *The Strange Career of Jim Crow* (New York, 1955).

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the heart of the matter. His glimmer of hope seems a faint light beside the massive fact of consistent discrimination.

Woodward's desire to dissociate the South from an ineluctable conservatism was to be further expressed in 1964. In an address at William and Mary College he spoke of tension in the contemporary South, a "crisis of identity." He deplored the stereotype of the conservative South and pointed to examples of Southern dissent, like the abolitionism that existed below the Potomac before the Civil War, and the writings of George W. Cable after it. "It would be a tragic decision to make intransigence and desperate adherence to a discredited code the test of southern loyalty."[33] Again he insisted that liberal alternatives were sanctioned by the lessons of the Southern past.

By the time Woodward offered these thoughts he had secured his present stature by publishing his *Origins of the New South, 1877–1913*, the massive embodiment of his thought. It is a complex book. Not the least of its merits was its closing of a gap in the historiography of the South. On the eve of its publication in 1951 Woodward himself wrote of a new volume on Mississippi politics:

> Falling between the period when the historians generally leave off and the period when the sociologists take over—between the end of Reconstruction and the very recent past—the half century studied by Mr. Kirwan represents the most neglected cycle of Southern history. It is therefore a compliment of a dubious sort to say that he has written the best political history of the period covered so far available for any state of the region. The fact is he did not have a lot of competition.[34]

Woodward's bibliography for his general study, itself the first of its kind, was a tale of woe: "Biographies of this period have only recently emerged from the commemorative stage and published correspondence is all but non-existent," he lamented, and then added, "A list of prominent figures of the post-Reconstruction South who have yet to find competent biographers would probably be longer than a list of those who have been so fortunate." Seldom can a subject have been raised from such obscurity to such illumination at a single bound. Just as a piece of technique, an effort of research, it was a virtuoso performance.[35]

More than that, it was the fulfillment of his new image of the Southern past, the fresh moral geography at which he had hinted in the 1930s. And its vision is moral. It is maintained throughout the work with a tenacity whose coherence makes it one of the few works of art that Southern historical literature has produced. It is expressed not merely in conclusions but in the structure and style of the analysis.[36]

[33] Woodward, "The Question of Loyalty," *American Scholar*, 33 (1964): 561–67.

[34] Woodward, review of Albert D. Kirwan's *Revolt of the Rednecks: Mississippi Politics, 1876–1925*, in *AHR*, 56 (1950–51): 918.

[35] Woodward, *Origins*, 482, 499. "The historian is driven to manuscripts by necessity rather than by zeal."

[36] Thinking of a parallel, the name that springs to my mind is not that of a historian but of

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Woodward's favorite mood is ironic. He is too scrupulous of the tradition of objectivity—so potent among American historians—to express explicitly his moral sensibilities. Rather he takes comfort in irony. The more it was used in *Origins of the New South,* the more it was clear he didn't approve of something. It was heaviest in his account of the men of the New South and the Redeemer regimes. Of Robert Dabney, for example, Woodward commented:

> Never, of course, was there the remotest chance of Dabney's goose quill prevailing against the clattering presses of Grady and Dawson, Tompkins and Edmonds. Anyway, the New South had no ear for pessimism—not with Georgia boasting eleven millionaires in 1892, and Kentucky twenty four, and New Orleans alone thirty-five![37]

Or note his scathing characterization of the myth of the Old South, created in the 1880s:

> What bittersweet tears washed Nashville's grimy cheeks over Page's *In Ole Virginia*! "Dem wuz good ole times, marster—de bes Sam ever see! Dey wuz in fac'! Niggers didn' hed nothin' 'tall to do." Embarrassing race conflict dissolved in liquid dialect, angry Populist farmers became merely quaint in Billy Sanders' vernacular, depression rolled aside, and for a moment, "de ole times done come back again."

Such passages contrast strongly with the seriousness of tone in the chapters on Populism. One can sense the satisfaction in a sentence like: "Not until the New South was confronted by the Populists did it meet with a challenge that set it back on its heels for a spell."[38]

Woodward's major reservation on middle-class reform was defined in the very chapter heading "Progressivism—For Whites Only." He summed up his objections to the movement by writing: "Progressivism . . . no more fulfilled the political aspirations and deeper needs of the mass of the people than did the first New Deal administration."[39] And this phrase, "the deeper needs of the mass of the people," neatly expresses Woodward's basic moral precept.

A MAIN TRADITION of American historical literature has been its moralism. As one social morality has succeeded another, interpretation has supplanted interpretation with a remarkable fidelity. Recent changes in Re-

a novelist, Henry James. Sheldon Hackney, in "*Origins of the New South* in Retrospect," suggests a reminiscence of William Faulkner. Granted that a New Englander is a more unlikely choice, there is a certain fastidiousness, a considered reflection, in Woodward's prose that inclines me away from the Southerner.

[37] Woodward, *Origins,* 174. Irony seems the natural refuge of the dissimulating moralist.

[38] *Ibid.,* 167, 174.

[39] *Ibid.,* 395.

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construction historiography are a good example of this. New social concern over the Negro has directly inspired an alteration in the hagiography of Reconstruction politics; attention has shifted from the oppressed white Southerner, focus of the Dunning school, to the Radical Republicans.[40] One recalls David Donald's remark about his biography of Charles Sumner: "I hope that no one will accuse me of sympathizing with Negro slavery because I have not interjected a little moral discourse after each of Sumner's orations to the effect that he was on the side of the angels. Surely in the middle of the twentieth century there are some things that do not need to be said."[41] And one cannot help thinking that the absence of moral polemic may be a small mercy beside the apparently effortless assumption of the turpitude of slavery and the sense of the relevance of that assumption.[42]

Consistent with this, Woodward's writing represents a shift in the morality of Southern thought. He has turned its concerns from the bourgeoisie to the Southern masses. He has widened it to include the Negro, whose social inferiority had been a central assumption of Dodd's generation. He has raised the broad issue of social responsibility in Southern politics. And it is interesting to note what little outcry his views on race have generated among the Southern intelligentsia. It is hard to imagine that he would have escaped unscathed or proceeded so laden with honors if he had written twenty or thirty years earlier.

Origins of the New South was published in 1951. If one didn't glance at the copyright, that is not the date one might guess. Charles Beard's vision of American history as a record of social struggle—so potent an influence on Woodward—was not one to conjure with in the intellectual atmosphere of the 1950s. It was, after all, the heyday of consensus, of the attempt to find the common denominators of American culture. When Richard Hofstadter wrote of the "common climate of American opinion" and remarked that its existence "has been much obscured by the tendency to place political conflict in the foreground of history,"[43] he gave a new mandate to the historical profession in America that Woodward, for one, ignored. Right in the middle of Woodward's foreground was the angry Populist farmer.

Woodward kept up a running dialogue with the consensus perception, until the events of the 1960s reasserted the impression of social division. He insisted upon the validity of dissent, whether of the economic class from a business culture or of the South from the national pattern. In 1959 he published a critique of the new hostility toward Populism, partly

[40] The Civil War itself has yielded to the same logic. See Thomas J. Pressly, *Americans Interpret Their Civil War* (Princeton, 1954).

[41] David Donald, *Charles Sumner and the Coming of the Civil War* (New York, 1960), x.

[42] The assumption may even involve an unconscious distortion, simply by omission. It is sometimes charged of the New Left, for example, that it is didactic and moralist about the past. But there is surely no fundamental distinction between Woodward's condemnation of the Redeemer regimes for their abdication of social responsibility and the New Left's charge that the New Deal was wrong in refusing to overhaul the American capitalist structure.

[43] Richard Hofstadter, *The American Political Tradition and the Men Who Made It* (New York, 1948), vii.

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exemplified in Hofstadter's *The Age of Reform*. Woodward conceded a portrait of nativism among Populists, its hatred of the Jew and the foreigner, its provincialism. But the Arkansan in Woodward clearly rose against the New Yorker in Hofstadter. He pointed out that these were not characteristics peculiar to Populists nor were they always true of Populists themselves. He referred to the case of Tom Watson, seeing positive value in this tradition of revolt.

> One must expect and even hope that there will be future upheavals to shock the seats of power and privilege and furnish the periodic therapy that seems necessary to the health of our democracy. But one cannot expect them to be any more decorous or seemly or rational than their predecessors.

Moreover, the intellectual has a role to play, as he did during the New Deal: "For the tradition to endure, for the way to remain open, however, the intellectual must not be alienated from the sources of revolt."[44]

Later Woodward returned more explicitly to the consensus school in an essay called "The North and the South of It." He asserted that "what America lacked in the way of class tensions she made up in the indigenous tensions of her peculiar heritage. Americans have characteristically thought in terms of regional, religious and racial or ethnic rather than class conflict." He added, "We have been somewhat hasty in sweeping under the rug of liberal consensus this ancient question."[45]

So Woodward was deliberately out of step. His *Origins of the New South* had its roots in the 1930s and, beyond that, in the 1890s.[46] However much Woodward's magnum opus marked a beginning for Southern thought, it stood at the end of a national tradition. In many ways it was the last triumph of the progressive school of Beard and Turner. He carried that doctrine to its logical conclusion, purged it of its naivety and methodological shortcomings, and wrote with a better style. Indeed, it was so well done, the ring of the prose so contemporary, that one forgets what kind of book it is. One is beguiled into neglecting that as Southern history it is probably a dead end.

If this is true, the responsibility lies ironically with Woodward himself. In *The Burden of Southern History*, written in 1960, he tried to define the distinctiveness of the South. Two essays, "The Search for Southern Identity" and "The Irony of Southern History," are particularly important. Woodward examined previous attempts to capture the essence of the South: Ulrich Phillips, who saw the resolution to maintain white supremacy as the cardinal test of the Southerner; and the Agrarians, for whom the South was the agricultural way of life. He found both to be at fault.

[44] Woodward, *The Burden of Southern History* (Baton Rouge, 1960), 166. This essay was originally published in the *American Scholar*, 28 (1959): 55–72.

[45] Woodward, "The North and the South of It," *American Scholar*, 36 (1966): 648.

[46] Consensus historians also had some of their origins in the 1930s, but they reflected the impulse of the New Deal whereas Woodward looked to the human agony of the Depression.

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One can be a Southerner without believing in black inferiority—on Phillips's logic, Woodward himself would not be a Southerner, an obviously intolerable conclusion—and to live in a city like Atlanta does not disqualify one from that identity. What then is different about the South? Woodward pointed to its variance from the norm of American experience. As the nation has been characterized by economic abundance, success, and the legend of innocence, so the South, in counterpoint, has been a land of poverty, failure, and the bitter realism born of such experience. "In that most optimistic of centuries in the most optimistic part of the world, the South remained basically pessimistic in its social outlook and its moral philosophy."[47] In a nation without a heritage of feudalism the South had slavery. In a country rootless with social mobility the South and its literature are firmly tied to place. But what is the use of this peculiarity? Scarred like Europe with war, the South has a sense of realism. It may be that the irony of the Southerner's position may make him more adept in an international world where American innocence is no asset.

In a revised edition of *The Burden of Southern History* Woodward was to return to this idea of irony. At the end of the 1960s he was forced to note how that decade's events had dismantled the national myth of innocence and success. His case might have been strengthened if the South had been innocent of the disorder of Vietnam and the black revolt, but

> the irony of history had caught up with the ironist—or gone him one better. For in this fateful hour of opportunity history had ironically placed men of presumably authentic Southern heritage in the supreme seats of national power—a gentleman from Texas in the White House and a gentleman from Georgia in the State Department. And yet from those quarters came few challenges and little appreciable restraint to the pursuit of the national myths of invincibility and innocence. Rather there came a renewed allegiance and sustained dedication.[48]

The curious thing about Woodward's initial essays on Southern identity, tacitly recognized in the revision, was his reliance on the consensus theory—the same idea he had so resisted in his own work on the South. However much the region had bred in him a pessimistic skepticism, it departed when he raised his eyes to the nation. He accepted the homogenized version of the American past contained in books like David Potter's *People of Plenty*. That assent is itself a comment on his theory of irony, as much as the labors of Lyndon Johnson and Dean Rusk. If his concept of the South had worked, he would have seized upon the consensus image and shown how much it ignored. That he accused the consensus image of misrepresenting the South was not quite enough. His insight about the South gave him no advantage over Northern historians in seeing suffering north of the Potomac. In truth it could not. His idea needed the counterpoint of Southern realism

[47] Woodward, *Burden*, 21.
[48] Woodward, *Burden* (rev. ed.; Baton Rouge, 1968), 230.

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and Northern innocence, Southern poverty and Northern wealth. Without it the distinctiveness of the South melted away. And it was his task to establish that singularity.

As the 1960s dealt savage blows to the consensus school so it undermined Woodward's definition of the South. He was left only with the conclusion that "Americans might still have something to learn, if they would, from the un-American and ironic experience of the South with history."[49] With that phrase, "if they would," his case slipped away. Woodward could only claim that, although there are lessons in the Southern past, they are there only for those who care to learn them. Thereby he lost the one point that would prove his theory: something in the Southern milieu compels Southerners to learn these lessons; it is the unavoidable burden of Southern identity. The scars of slavery and Reconstruction now have an academic status as potent as that of the Punic Wars—no more.

The cause of Woodward's error is clear. He was misled by the logic of his own Southern liberalism. He tried to fuse two separate strands of the Southern mind that have seldom mingled save in his own mind. Those Southerners most marked by the pessimistic resistance to panacea of which Woodward approves are not liberals. They are Southern conservatives. It is they who have felt most sharply the sting of defeat and drawn lessons from it. But they were conservative lessons; Southern liberals took the same events with comparative lightness because they were not intellectually involved in them. If one examines the genteel historians whom Woodward himself supplanted, one sees that the very things they ignored in the Southern past are those episodes richest in social disappointment. Their task, after all, was the Southern future and a judicious edition of the Southern past, not the responsibility for Jefferson Davis's aberrations.[50]

Their vision of that future was euphoric. Liberals like Walter Hines Page were constantly heralding the dawn of a new age. Edwin Mims's *The Advancing South* is as far removed from social pessimism as one can imagine: "No one can have too high a hope of what may be achieved within the next quarter of a century. Freed from the limitations that have so long hampered it, and buoyant with the energy of a new life coursing through its veins, the South will press forward to a new destiny."[51] A main task of Southern liberalism has been to assimilate the South to the nation. It has

[49] *Ibid.*, 233.

[50] It is important to note that Woodward's Populist strategy has been untypical of recent Southern writing. Historians have generally preferred to revitalize the middle-class heritage. In particular one thinks of several pupils of Fletcher Green at the University of North Carolina, like Arthur Link, Charles Sellers, and Dewey Grantham. Their "manifesto," *The Southerner as American* (Chapel Hill, 1960), shows their preoccupation with the issue of nationalism, a central part of the New South tradition. In so far as disciples have been few, Woodward the "Populist" might be regarded as an aberration. See Woodward's review of Charles Sellers, ed., *The Southerner as American*, in *Journal of Southern History*, 27 (1961): 92–94. That Woodward originally wrote "The Search for Southern Identity" for inclusion in *The Southerner as American* does not, I think, substantially alter the point even if it untidies the symbolism.

[51] Mims, *Advancing South*, 315.

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been an impulse toward, not away from, innocence. If the Vietnam misadventure can be seen as an exercise of American innocence, the role of Lyndon Johnson in it is entirely faithful to that Southern tradition of liberalism. He is more typical of it than Woodward himself.

Woodward's essay on Southern identity nevertheless represented an immense leap forward in the debate about the South. He cut away the idea that the South is to be identified with any one political or economic institution. In so doing, however, he destroyed the basic impulse of Southern historiography, the desire to examine certain moments and situations in the Southern past to glean the inner meaning of the South. And thereby he destroyed the rationale of his own *Origins of the New South,* the most brilliant essay of the genre, by making redundant its Populist climax. Woodward raised the issue of Southern identity as the central theme of Southern history.[52] He continued the false logic of Ulrich Phillips and the Agrarians, however, by trying to tie that identity to one sensation rather than another. He picked on the experience of poverty and defeat and leaped to the premature generalization that it made the South distinct. This is clearly as much of a half-truth as Phillips's belief that white supremacy made the South. Is one to exempt all Southerners with no grim sense of human frailty from the community of the South? If so, one would have to exempt most of the Southern liberal tradition.[53]

These essays by Woodward are not merely an attempt to comprehend the Southern experience. They are also the best contemporary examples of what may be the only central theme, the search for the identity itself. Being a Southerner may be no more—and certainly no less—than accepting that identity and using it. What one does with it can vary enormously. The images one holds of oneself, the Southerner, and the South are prey to widely divergent social influence.

To appreciate the centrality of this search for identity and Southerners' odd lack of awareness of it, one has only to look at Southern historiography. This is the final irony. If the South is a habit of thought, it would follow

[52] The Southern literary "renaissance," of which, in some ways, Woodward is a part, has been more distinguished by its social conservatism than by its liberalism. It may be relevant to note that Robert Penn Warren, a close friend of Woodward's (*The Burden of Southern History* is dedicated to him), is unusual in having progressed from a conservative to a liberal position, just as Woodward is singular in having tried to fuse the two traditions.

[53] I distinguish, of course, between "Southern history" with its implicit commitment to resolving the riddle of the South as an entity, and the simple history of the Southeastern United States, which has no overall conceptual problems. One should note, in passing, that Woodward has published one major book since 1960, *American Counterpoint: Slavery and Racism in the North-South Dialogue* (Boston, 1971). Most of the essays are slightly altered reprints and do not much affect the drift of my remarks. Important, however, is the preface, in which he offers some new thoughts on Southern identity. He has moved from the naked perception of *The Burden of Southern History* to some exploration of the mechanism of the identity, in the context of Southerners' perception. It is a logical corollary to having shifted the definition of the South from its politics to its mind. He points to the "counterpoint" between North and South and calls for a history of "North-South images and stereotypes, of when and how and why they were developed." This viewpoint is, however, an apparent afterthought to the book and cannot be said to control its structure.

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that Southern history is pre-eminently the history of the Southern mind. But if one looks at the groaning shelves of books on the South, one finds thousands of volumes on its politics, hundreds on its economics and its sociology. One does not find a significant body of work on its intellectual history. There are a few books on the antebellum mind, one or two good things on the New South creed and a reasonable batch of studies on the literary scene since the First World War, but no more. It seems odd, but the answer is simple enough. Southerners have not written their intellectual history because they have not thought the South defined by its mind. The South was a matter of race or politics or economics but not the pattern of its thought. Southerners have been so intent upon the search itself that they have neglected to notice they are out hunting. Even Woodward remained transfixed by the mirage at the end of the chase, the final proof of what is different about the South. He has expressed concern that with the end of Southern distinctiveness in politics and economics, the need for a Southern identity will evaporate.[54] His own case suggests that he need not be concerned as long as Southerners believe the search to be worth the effort. And there is no evidence that their zeal is flagging.

[54] Woodward, *Burden*, 5.

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Female Emancipation, Birth Control, and Fertility in European History

EDWARD SHORTER

WE HAVE KNOWN for some time that late in the nineteenth century legitimate fertility declined. We have recently learned that late in the eighteenth century legitimate fertility of young women rose. We have more recently discovered that during both periods illegitimate fertility moved parallel to legitimate.[1] Yet to date we have succeeded in explaining none of these movements. Despite more than a generation of scholarly attention, no satisfactory answer has emerged to explain the plunge of the birth rate on the threshold of the twentieth century. And why, say, illegitimacy climbed late in the eighteenth century has been as puzzling to historical demographers as the making of gold once was to alchemists. Little progress has been made in these matters, it seems to me, for two reasons. We have persisted in treating these various secular movements in fertility as separate, discrete phenomena, rather than as part of a unified whole, and we have paid insufficient attention to the history of that group of the population most closely involved with fertility—women.

The purpose of this article is to present an argument linking the history of women to this parabolic rise and fall of fertility. Now at first this seems like the most natural sort of explanation: to account for changes in fertility with hypotheses about the changing nature of feminine attitudes or about the woman's place within the family. Yet in the past professional

As a result of the helpful and often severely critical comments of Lutz Berkner, Ansley Coale, Natalie Davis, John Gillis, Bea Gottlieb, John Knodel, William Langer, Joan Scott, William Sewell, Daniel Smith, and Etienne van de Walle, the present article differs substantially from an earlier version presented at the annual meeting of the American Historical Association, New York, December 28, 1971.

[1] On the fall in marital fertility, see Ansley J. Coale, "The Decline of Fertility in Europe from the French Revolution to World War II," in S. J. Behrman *et al.*, eds., *Fertility and Family Planning: A World View* (Ann Arbor, 1969), 3–24; for a recent case study of the eighteenth-century fertility rise, see E. A. Wrigley, "Family Limitation in Pre-Industrial England," *Economic History Review*, 2d ser., 19 (1966): 82–110; and on the late-nineteenth-century and twentieth-century decline in illegitimate fertility, see Edward Shorter, John Knodel, and Etienne van de Walle, "The Decline of Non-Marital Fertility in Europe, 1880–1940," *Population Studies*, 25 (1971): 375–93.

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demographers have monopolized the study of fertility, and they have been reluctant to venture beyond their technical indexes of vital events to the fabric of popular life, where the fundamental explanations of demographic change lie. Provincialism has equally marked the other side. Writers on women's history have been finely attuned to belletristic evidence, yet loath to employ either solid demographic data or to borrow arguments and techniques from the social sciences in general. As a result people who might have resolved convincingly some of the big questions of family and demographic history, had they joined forces, have sailed past each other like two ships in the night.

WHAT, EXACTLY, IS THE PROBLEM to be explained? We may distinguish among four separate movements in the great parabolic curve of fertility history. There were two upward thrusts during the eighteenth and early nineteenth centuries, one for illegitimate, the other—somewhat truncated by age group—for legitimate fertility, and then two downward slides, late in the nineteenth and early in the twentieth centuries, for both marital and nonmarital fertility among women of all ages.

In the course of the eighteenth century, and in some places during the first half of the nineteenth as well, a substantial increase occurred in the number of children a married woman would be likely to have in her twenties. The increase centered among the 20–24 year-olds, but commenced sometimes among the few women married between 15 and 19, and continued on occasion to the 25–29 age group. Although not universal, the increase was widespread: in thirteen of the sixteen French villages on which scholars have made available suitable information (age-specific marital fertility rates—see appendix 1), the birth rate rose among 20–24 year-olds. The same happened in all five of the German villages studied to date. Contemporary historical demography has been much less attentive to other parts of Europe, but E. A. Wrigley's work on Colyton in Devon evidences the same eighteenth-century increase (extending to 35–39 year-olds), and national rates among young Swedish women also turn upward (see appendix 1). The reader must bear in mind that we are not dealing with a general increase in marital fertility, for the number of children born to women over 30 either remained steady or drifted slowly downward after 1700. (The reader should also note that the increase in the age-specific marital fertility rates cannot be attributed to earlier ages at marriage, for the statistics represent births as a proportion of the number of women already married rather than as a proportion of the total number of women. Thus a decline in the age at marriage might increase total fertility by increasing the number of sexually active women, but would not necessarily affect the fertility of women in any specific age group.)

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What we have before us is some change in the behavior patterns of young women, rather than an across-the-board shift in fertility.

Most surprising among recent research findings is a sharp increase in illegitimate fertility late in the eighteenth century.[2] Here we are plagued by evidential problems. Not knowing how many unmarried women there were in the population—and how their proportion changed from year to year—prevents the calculation of illegitimate fertility rates (the number of illegitimate births per 1,000 unmarried women, ages 15–45). Rather, parish-register data give us access to the illegitimacy ratio alone, that is, the number of illegitimate children baptized relative to the total number of baptisms. This guide, as a measure of the propensity to illegitimacy, may be subject to two kinds of distortions. First, changes in the number of children born within wedlock: if the legitimate birth rate should fall, illegitimate children will loom larger in the ratio simply because the denominator of the ratio has grown smaller, without the numerator (the number of children born outside of wedlock) having changed at all. Second, changes in the number of unmarried women in the population: if there should be more single women, the number of illegitimate children may well increase, thereby causing the ratio to rise. But the propensity of these women to have unsanctioned sexual liaisons may not have altered at all; it is simply that there are more such women available to produce illegitimate children.

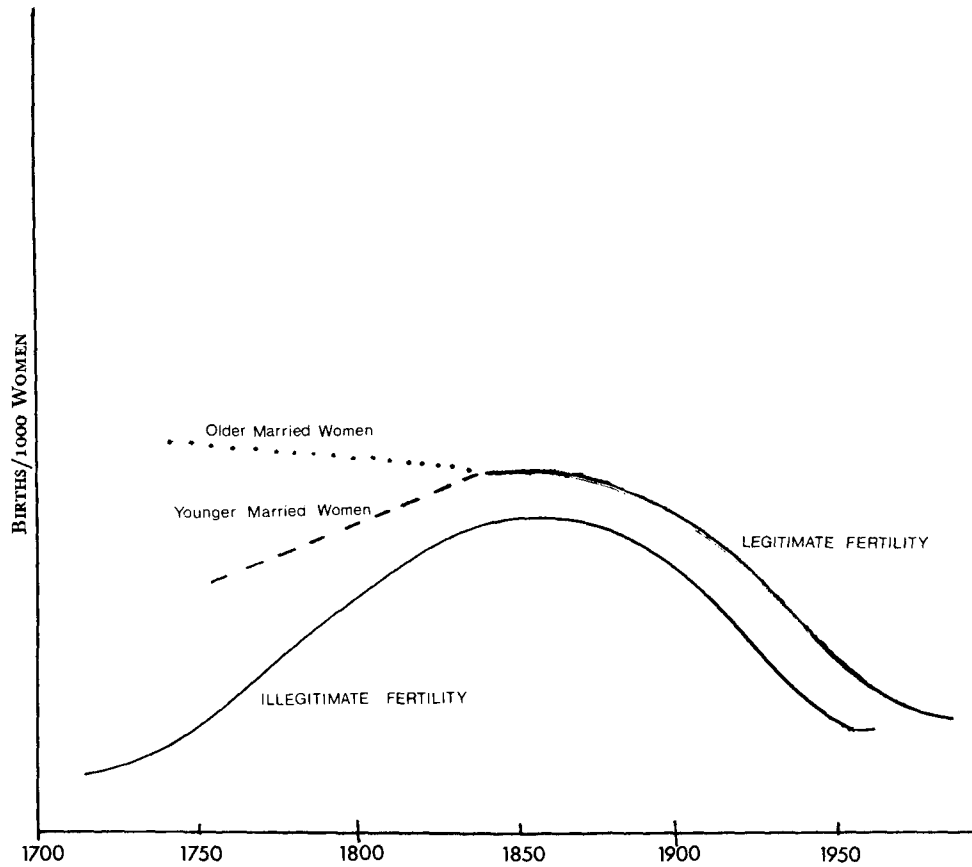
Yet the rise in illegitimacy ratios all across eighteenth-century Europe and England (and the colonial United States) is so large as to make clear that the rate was increasing as well. In some places the population of unmarried women expanded because of a higher age at marriage, yet not to the extent that illegitimacy grew. In other places where illegitimacy increased, the age at marriage fell, decreasing the population of unmarried. Possibly a decline in the number of children born to older married women caused the same number of bastards to loom ever larger. Yet here again, the fourfold and fivefold leaps in the illegitimacy ratio one commonly encounters suggest that statistical artifacts must be ruled out and that, instead, the propensity of unmarried women to bear illegitimate children (i.e., illegitimate fertility, properly understood) had in fact increased.

As figure 1 further suggests, the illegitimate fertility rate soared between 1750 and 1850, from one end of Europe to another. In all but a handful of villages and cities for which data are available, illegitimacy

[2] Local series on trends in illegitimacy are reviewed in Edward Shorter, "Illegitimacy, Sexual Revolution and Social Change in Europe, 1750–1900," *Journal of Interdisciplinary History*, 2 (1971): 237–72. It would be superfluous to cite here the long lists of articles and monographs containing time-series illegitimacy ratios; from this evidence one may conclude the level of illegitimacy greatly increased late in the eighteenth century. Age at marriage is a standard item of information in the French local demographic studies. A convenient guide to this literature, for readers who wish to pursue the matter further, is Jacques Dupâquier, "Sur la population française au XVII[e] et au XVIII[e] siècle," *Revue historique*, 239 (1968): 43–79.

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FIG. 1. LIKELY COURSE OF FERTILITY IN MODERN EUROPE



rose, departing from modest plateaus of one to three per cent of all baptisms, to often ten or fifteen per cent. Also prebridal pregnancy—that is, women who are already pregnant when they marry—climbed dramatically. We are able to determine from parish-register data what percentage of all first children were born less than eight months after marriage. And this percentage increases along with illegitimacy in most places.[3] Thus not only were couples who did not marry having premarital intercourse more often than before, couples who did marry were doing so as well.

What "intervening" variables in the chain of demographic events from intercourse to live birth caused this increase in nonmarital fertility? Was it, perhaps, that fewer abortions or increased fecundity produced the rise? Or a decline in something as simple as the stillbirth rate? Of the various explanatory possibilities a demographer might advance, I argue that an increased exposure to intercourse was most important. In other

[3] For data on prebridal pregnancy, see appendix 2. Series reported in Shorter, "Illegitimacy, Sexual Revolution and Social Change in Europe," show the simultaneity of the increase in prebridal conception and illegitimacy.

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words, the illegitimacy explosion probably took place because more young people than ever before were having sex before marriage. This heightened sexual activity meant that, inevitably, children were conceived out of wedlock.

Then, around the middle of the nineteenth century in some regions, and as late as the end of the century in others, a great slide in both varieties of fertility commenced. The startling plunge in marital fertility after the 1880s has long been documented. In province after province across the Continent married women began having fewer children.[4] The large families of four and five children so characteristic of, for example, early modern Germany, were giving way to the *Zweikindersystem*. In the nineteenth century publicists and politicians, fearing an attrition of potential military recruits, sounded the alarm. In recent years the decline in marital fertility has become an object of intensive scholarly study, for as challenging as the question of public policy—What to do about it?—is the intellectual question, Why did it happen?[5]

To complete the downslope of the parabola, the parallel decline in nonmarital fertility between around 1880 and 1940 must be mentioned. With approximately the same timing and pacing as marital fertility, illegitimacy dropped off across Europe and by 1940 had in almost every province been cut at least in half. Unlike the marital-fertility decline, this slide in illegitimacy has only been recently noticed.[6] Consequently we have no fundament of previous scholarly work to build upon.

Almost by process of elimination we are able to single out birth control as the chief intervening variable in the fertility decline. Changes in the

[4] The Office of Population Research (OPR) at Princeton University is, under the direction of Professor Ansley J. Coale, currently concluding a massive investigation of the fertility decline. Its researchers have assembled for more than seven hundred province-level units time series on fertility that control, when possible, for changes in the age and marital distribution of the female population. The OPR has most generously permitted me to see some preliminary findings from this study, and all references to the drop in European fertility, unless otherwise noted, are based on OPR data. Among the publications of OPR researchers on changes in Europe's fertility are Coale, "The Decline of Fertility in Europe"; Paul Demeny, "Early Fertility Decline in Austria-Hungary: A Lesson in Demographic Transition," *Daedalus*, 97 (1968): 502–22; Massimo Livi Bacci, *A Century of Portuguese Fertility* (Princeton, 1971), "Fertility and Population Growth in Spain in the Eighteenth and Nineteenth Centuries," *Daedalus*, 97 (1968): 523–35, and "Il Declino della fecondità della popolazione italiana nell' ultimo secolo," *Statistica*, 25 (1965): 33–35; Etienne van de Walle, "Problèmes de l'étude du declin de la fécondité européene," *Recherches économiques du Louvain*, no. 4 (1969): 271–87; van de Walle and John Knodel, "Demographic Transition and Fertility Decline: The European Case," International Union for the Scientific Study of Population, *Contributed Papers, Sydney Conference, August, 1967* (Sydney, 1967), 47–55. John Knodel has kindly permitted me to read his forthcoming monograph on the fertility decline in Germany, *The Decline of German Fertility between Unification and the Second World War* (Princeton, 1973).

[5] For a recent state-of-the-problem summary, see R. Lestaeghe, "Le Dossier de la transition démographique," *European Demographic Information Bulletin*, 1 (1970–71): 218–29.

[6] The illegitimacy ratio had obscured it, for as marital and nonmarital fertility fell off at about the same speed, the ratio remained constant. See Shorter, Knodel, and van de Walle, "Decline of Non-Marital Fertility in Europe." Phillips Cutright has also mentioned the simultaneity of the decline in "Illegitimacy: Myths, Causes and Cures," *Family Planning Perspectives*, vol. 3, no. 1 (1971): 25–48.

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other steps to motherhood seem unlikely candidates for a shift of such magnitude in fertility. Let us consider them systematically.

Was there, for example, less exposure to intercourse, and therewith to impregnation? Nobody knows, but it does not seem likely.[7] Or was fecundity reduced, so that women became less able to conceive? Changes in this intervening variable seem to have gone in exactly the opposite direction, with lower ages at menarche and better diet leading to improved fecundity. Thus we may rule out, for the European population at least, explanations emphasizing growing sterility.[8] Did the incidence of involuntary pregnancy interruptions rise, so that if women had fewer children, it was because they spontaneously aborted more often? Probably not. Quantitative indicators on such matters are rare and difficult to interpret. But if one may regard trends in stillbirths as a surrogate for trends in premature pregnancy interruptions, the change over time was toward fewer foetal deaths, not more.[9] This trend would have enhanced fertility rather than decreasing it.

Thus an analysis of the intervening demographic variables suggests, if only by elimination, that the principal variable behind the fertility decline was birth control.[10] What remains uncertain in the decline is the relative balance of each of the two components of birth control—contraception and abortion. The constellation of social forces behind the use of coitus interruptus, as I shall later suggest, may well have differed from that behind abortion. And if widespread abortion turns out the principal

[7] Several scholars currently working on the anti-intercourse literature of mid-nineteenth-century North America tentatively suggest, however, that a large number of men were in fact moved by such publicists as Sylvester Graham to ration their semen carefully. See for example Stephen Nissenbaum, "Careful Love: Sylvester Graham and the Emergence of Victorian Sexual Theory in the United States, 1830–1840" (Ph.D. dissertation, University of Wisconsin, 1968); Michael Bliss, "Pure Books on Avoided Subjects: Pre-Freudian Sexual Ideas in Canada," Canadian Historical Association, *Historical Papers, 1970* (Ottawa, 1970), 89–108; and Ben Barker-Benfield, "The Spermatic Economy: A Nineteenth-Century View of Sexuality," *Feminist Studies*, 1 (1972): 45–74. Whether this variety of male abstinence played a role in the North American fertility decline remains to be investigated. On social ideologies stressing sexual continence in Europe, see Peter C. Cominos, "Late-Victorian Sexual Respectability and the Social System," *International Review of Social History*, 8 (1963): 18–48, 216–50.

[8] On a secular decline in the age at menarche, see most notably J. M. Tanner, "The Trends towards Earlier Physical Maturation," in J. E. Meade and A. S. Parkes, eds., *Biological Aspects of Social Problems* (Edinburgh, 1965), 40–65. Reynolds Farley suggests that disease and malnutrition may have depressed the fecundity of American blacks; their birth rate fell off at the same time as the white birth rate, yet there is no evidence that before 1940 blacks in any number had been practicing birth control. *Growth of the Black Population: A Study of Demographic Trends* (Chicago, 1970), especially 206–45.

[9] In Norway, for example, stillbirths decline steadily from 50.6 per 1,000 births in 1876–80 to 35.4 in 1921–25 and then, after fluctuating a bit, to 22.5 in 1961–65. Norway, Statistisk Sentralbyrå, *Historisk Statistikk, 1968* (Oslo, 1969), 54.

[10] This is certainly the received wisdom, and no new findings have turned up to challenge it. David Glass, for example, writes: "The decline in fertility in Great Britain since 1870 can be explained largely, though not completely, in terms of the increased practice of deliberate family limitation." *Population Policies and Movements in Europe* (Oxford, 1940), 61. Glass stresses for many countries the importance of abortion alongside contraception in regulating the decline. See, for example, his discussion on page 279 of German fertility.

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regulator of fertility, the true explanation of why fertility dropped may depart radically from the conventional wisdom.

The features that must be emphasized about both components—marital and nonmarital—of the fertility parabola are their parallel trends and their simultaneous movement across territory. The fertility of young married and unmarried women seems to have moved in unison on the upward slope, and the fertility of older married and unmarried women changed together on the downward slope. Excellent nineteenth-century information in particular demonstrates irrefragably a downward march in lock step of both fertilities. This is true at least at the national level, although more divergence may be noted at the provincial. France, as in so many demographic characteristics, deviates from the rule of parallelism, for illegitimacy there never crashed bottomward in ski-jump style and French marital fertility had begun a downward drift at the beginning of the nineteenth century rather than at the end. As for simultaneity, in most of Europe fertility movements were synchronized within two or three decades—a very short period of time, as secular demographic trends go. The illegitimacy rise commenced almost everywhere within the period from 1750 to 1780; and with even more precise timing, the sharp downturn in both marital and nonmarital fertility began between 1860 and 1890 in most provinces on the Continent. Within a given country, such as Norway or Germany, the timing would normally be closer still. This striking simultaneity suggests that the fertility decline, for example, did not come about as different populations encountered successively the massive structural changes of modernization. The falling birth rate would thus not appear as merely the result of increasing migration to the city or of growing factory employment. Rather it seems likely that the mentalities and cultural operating rules that dictated fewer children to millions of families, and to millions of single women, spread through some process of diffusion. In short, regardless of the specific structural features of an area—whether peasant subsistence farming or agricultural capitalism, whether commercial trading or large-scale factory industry—the fertility decline would come to it within a relatively short span of time.

Having defined the problem and the logical limitations within which a successful explanation will have to maneuver, I will now propose a model designed to account for the parabola's parallelism, paying less attention to the matter of simultaneity, yet attempting to show how two competing views on the diffusion of the fertility decline might be reconciled. The following argument should be regarded as speculative, not as a demonstration of hard and fast fact. Sophisticated research into these vexing questions has only begun, and the accumulation of evidence and hypotheses on the social sources of fertility may well require years. Yet quite apart from their rightness or wrongness, such preliminary arguments may deserve to be advanced for several reasons. They call attention to the unitary, co-

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hesive nature of a set of phenomena hitherto regarded as unrelated, hitherto indeed only partially identified as phenomena in need of explanation at all. The following argument has also the virtue of not being inconsistent with any important existing finding on the subject. It attempts to organize what we now know into a coherent framework, so as to better identify what we as yet do not know.

Two massive secular forces regulated the fertility parabola: a growing sense of personal autonomy and independence among women and the diffusion of techniques for contraception and abortion. The former change represented "female emancipation" among the popular social classes, for it involved a new frame of mind in which women saw themselves increasingly independent of parental and husbandly authority, masters of their own emotions and, ultimately, of their own fertility. This sense of personal autonomy was probably initiated by structural shifts in the economy. The second great force for change was the adoption of birth-control techniques. Like the mental state of personal autonomy, it came initially in consequence of structural changes in social institutions, changes that compelled people to adapt their personal situations to new conditions of life. Yet later, birth control was carried through a momentum of its own via the mass media and the informal social networks of the city to the Continent's millions of anonymous women.

Paradoxically, these grand movements began among entirely different clienteles, at opposite ends of the spectrum of age and status. The sense of personal autonomy commenced among young, lower-class women and spread upward to older and more prosperous groups. The birth-control movement, on the other hand, began in the eighteenth century among more mature middle-class women and only with time spread downward to young women in the lower classes.

Female emancipation was, in the absence of birth control, responsible for the upward slope of the nonmarital fertility parabola and for the increase in the marital fertility of those young women who entered marriage already pregnant. The birth-control movement, by way of contrast, affected initially only a limited audience and was without important statistical consequences, for the women who in the eighteenth century turned to contraception and abortion were not numerous in comparison to the popular classes of women. The point at which the birth-control movement intersected, in its downward course, the upward-bound women's emancipation movement was the point at which the massive fertility decline began. For only late in the nineteenth century did information on contraception finally make its way down the age and social hierarchies to the masses of women whose free and easy attitude toward sexuality had produced the fertility rise. Newly armed with knowledge of exactly how to curb their reproductive capacity, these women proceeded to sever the connection between intercourse and conception, so that both marital

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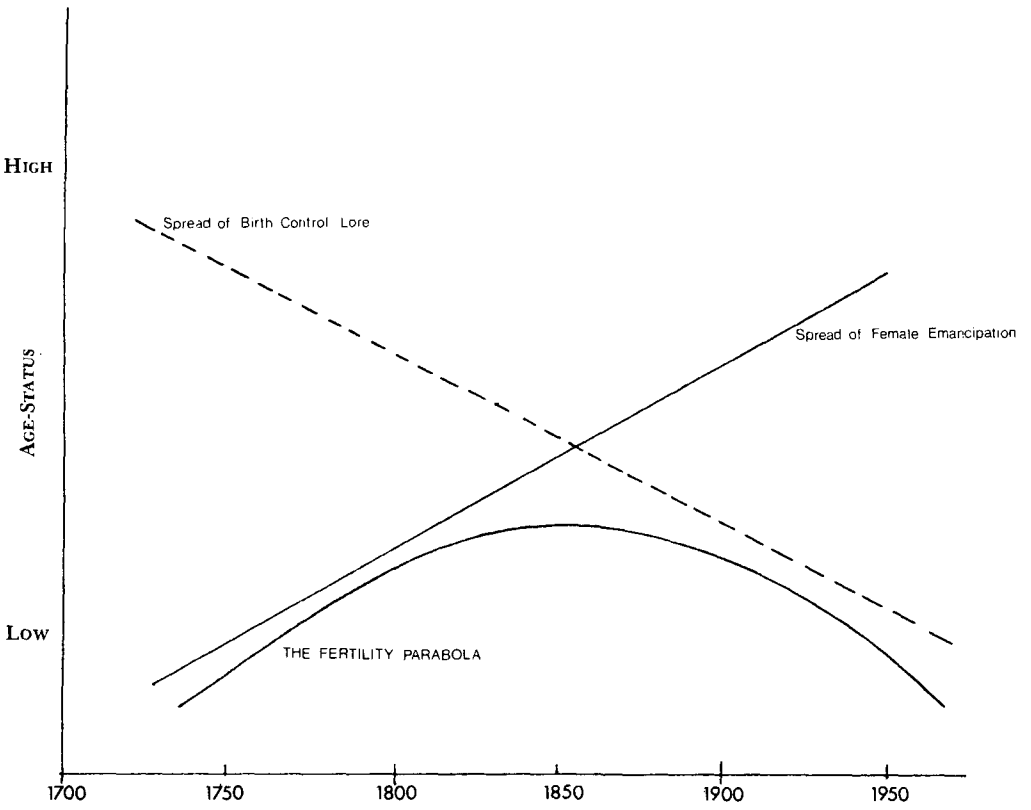
and nonmarital fertility dropped off. Thus the downward slope of the parabola took the place of the upward.

To dwell for just a moment on the complexities of the relationship between emancipation and fertility, I warn the reader that I intend to have my cake and eat it too. I intend to argue with sober responsibility that the increase in the fertility of young married and unmarried women was just the same phenomenon: young single women becoming more active sexually as a result of romantic love and emancipated revolt, so that the rise in marital fertility was a largely artifactual consequence of premarital intercourse, as young women who entered marriage pregnant boosted the fertility rates of the age groups in which they married. It is likely that, with time, emancipation spread upward to older age groups of women, whose weddings were many years behind them. Yet we shall not be able to follow these subsequent changes in outlook by way of fertility rates, because the influence of bridal pregnancy is exerted only upon the age group in which marriage occurs. That is the responsible position. The wildly speculative position—which I shall later present as an alternative worthy at least of consideration—is that increased frequency of intercourse within marriage as well as without also helped to elevate marital fertility. So *caveat lector*.

Figure 2 permits us to visualize these relationships more clearly. The horizontal axis of the graph represents time, the vertical axis the age-status hierarchy. The line representing the awareness of autonomy among women goes from lower left to upper right, indicating a diffusion over the years from younger women of the working classes to older middle-class women. The line representing birth-control sophistication runs from upper left to lower right, suggesting that over the years a practical understanding of contraception and abortion spread from older middle-class women—both married and unmarried—to younger working-class women. Emancipation in the absence of birth control resulted in the upward bound of fertility; emancipation in the presence of birth control elicited the fertility decline. Around 1750 few young women were aware of emotional stances toward their husbands and parents other than the conventional one of submissiveness and subordination, particularly in matters sexual, generally in matters of authority. Around 1750, furthermore, few such women were aware of the possibility of controlling their own fecundity or that children need not be a curse of God and intercourse a dreary obligation imposed by an exacting, all-powerful male society. By 1900, to take a date at which both great secular trends had substantially completed their diffusion, European women of all ages were prepared to assert their independence and emotional autonomy vis-à-vis parents and husbands and to ensure that the sex act would not necessarily mean impregnation. They had become persuaded that a great many children, or in the case of unmarried women any children at all, were an avoidable inconvenience.

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FIG. 2. HOW FEMALE EMANCIPATION AND BIRTH CONTROL AFFECTED FERTILITY



That is the argument. In the remainder of this article I should like to consider in more detail the motors and trajectories of each of these great changes.

THE CONVENTIONAL WISDOM about female emancipation is that it originated among upper-class women in the mid-nineteenth century, surfacing first in tandem with the movement for emancipation of the slaves, then moving forward independently as the suffrage movement. While this account may be substantially correct as involves women's participation in national political life, it is, in my opinion, inapplicable to family history.[11] I suggest that

[11] J. A. and Olive Banks, *Feminism and Family Planning in Victorian England* (New York, 1964), is a sophisticated recasting of this conventional wisdom. Kate Millett's *Sexual Politics* (New York, 1970) has again documented the thesis, using belletristic evidence. I have attempted, in choosing the phrase "female emancipation" as a description of changes in women's mentalities, to avoid the connotations of either of two alternative terms: "women's liberation," which moves too much what's-happening-now explosive into the vicinity of a basically scholarly discussion, and "feminism," which refers specifically to the agitation of upper-middle-class women for equal educational opportunities and the like. Two new studies of these educated, politically motivated feminists are William L. O'Neill, *Everyone Was Brave: A History of Feminism in America* (Chicago, 1971), and David M. Kennedy, *Birth Control in America: The Career of Margaret Sanger* (New Haven, 1970). My argument, of course, takes issue with this literature in emphasizing the working-class origin of the female emancipation movement, in singling out family rather than national politics as the field of agitation, and in dating the beginning about a hundred years earlier than is customary.

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the position of women within the family underwent a radical shift starting late in the eighteenth century; furthermore, the change progressed from young and lowly women to older women of higher status.[12] The logic of this chronology sees involvement in the economy of the market place as the principal motor of emancipation.

What exactly is meant by "female emancipation"? General statements about the position of women within early modern European families are uncertain in the extreme because, at the same time, so many impressions of individual famous women are to be found in the literature and so little is known in a systematic, quantitative way about the cultural rules and norms of women in the popular classes. Yet one might fairly characterize the situation of most women as one of subordination. In the first place, both young men and women were subordinated to the authority of their parents, so that parental intervention in the mating market customarily replaced romantic love in bringing young couples together. In the second place, both social ideology and the force of events conspired to make the husband supreme over the woman in the household, his obligation being merely to respect her, hers, however, to serve and obey him. In most matters of sex, economics, or family authority the woman was expected to do the husband's bidding. Clearly individual exceptions existed, yet the rule seems to have been powerlessness and dependency for the woman.[13]

Thus female emancipation involves, quite simply, the replacement of this subordination with independence. In the nineteenth and twentieth centuries married women acquired for themselves first, practical leverage on household political power, and second, a family ideology stressing their own rights to sexual gratification and emotional autonomy.[14] And

[12] In treating the "age-status" spectrum as a single continuum I am assuming that the young tend to behave like the lower classes do, the more mature like the middle classes. Subcultures of youth have in fact much in common with lower-class subcultures because the young, like the lower classes, tend to be present-oriented, while older people, like the middle classes, tend to be more oriented to the future. Maurice Agulhon's important study of Basse Provence shows this interaction between youth and lower-class cultures at work: "Thus it is adolescent sociability that probably served as model and point of departure for popular sociability in general, whose thoroughgoing diffusion took place in the nineteenth century, the Indian summer of the 'chambrées.' (The emotional affinity between youth and proletariat, an article of faith for traditional paternalists, turns up here in the form of an observable though somewhat hazy social phenomenon.)" *Pénitents et francs-maçons de l'ancienne Provence* (Paris, 1968), 250.

[13] I have relied partly on Helmut Möller's *Die kleinbürgerliche Familie im 18. Jahrhundert: Verhalten und Gruppenkultur* (Berlin, 1969), *passim*. See also J. M. Mogey, "A Century of Declining Paternal Authority," *Marriage and Family Living*, 19 (1957): 234–39. Eugen Lupri, who characterizes the "traditional" German family as extremely patriarchal, postulates a change to female emancipation over time. But for Lupri "traditional" means the period around 1900, and he has no evidence on the years before the First World War. "The West German Family Today and Yesterday: A Study in Changing Family Authority Patterns" (Ph.D. dissertation, University of Wisconsin, 1967).

[14] The powerful trend toward emancipation was reflected, at least for middle-class women, in the mirror of marriage-manual advice. See Michael Gordon, "From an Unfortunate Necessity to a Cult of Mutual Orgasm: Sex in American Marital Education Literature, 1830–1940," in James Henslin, ed., *Studies in the Sociology of Sex* (New York, 1971), 53–77; see also, Michael Gordon and M. Charles Bernstein, "Mate Choice and Domestic Life in the Nineteenth-Century Marriage Manual," *Journal of Marriage and the Family*, 32 (1970): 665–74.

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unmarried women became increasingly convinced of the impropriety of family and community restraints upon social and sexual relations, so that they came to ignore the strictures of both parents and community in order to gratify their own personal needs. Therefore women's emancipation at the popular level means disregarding outside controls upon personal freedom of action and sexuality for the sake of individual self-fulfillment.[15]

What evidence exists that the years 1750–1850 saw a movement toward female emancipation among the popular classes? We are, alas, at the beginning of the investigation rather than the end, and so I can merely anticipate the findings of future research. Yet even within the existing literature strong hints may be found that crucial changes in the status and authority of women were under way after 1750 and that these changes were linked in some way to economic modernization. The search for evidence may be aided by considering the nature of the change in the relationship between married woman and husband as well as that between the young, unmarried woman and parental and communal authority. To demonstrate that there is in fact an *explicandum,* let us briefly review some previous findings on these questions.

Least studied to date has been the family life and authority relationships of lower-class women in the years before 1900. Save for tiny pinpricks of information here and there the subject is uncharted, yet those studies that exist converge to demonstrate a radical upheaval in popular family life in the wake of capitalism. Neil Smelser, in a classic study of the British cotton industry, describes "the reversal of traditional age and sex roles as wives and children went to the factory." Industrial growth fragmented the customary "family economy" by making individual producers of its separate members. And, for the children at least, independence ac-

[15] Those observers familiar with working-class sexual patterns in North America will doubtless react with some incredulity to the notion of the lower classes in the vanguard of a sexual revolution. After all, on this side of the Atlantic it is precisely the working classes who count as most "repressed" sexually, who have the highest degree of role segregation and the least interest in eroticism as a form of personality development. Recent research has, however, made evident that the Continental pattern of working-class sexuality is entirely different: a close tie between coitus and romantic love, less pronounced sex roles for men and women, inventive and spontaneous styles of intercourse. Volkmar Sigusch and Gunter Schmidt, having discussed the "de-emotionalizing of sexuality, or of extreme double standards with dissatisfaction and desexualization of the female—characteristics . . . typical of the American lower classes," continue: "The West German workers regard sexuality much more as a social activity involving reciprocity. Mutuality should definitely be present in the nonsexual areas as well. The strong tendency toward assimilating sexuality with personal and emotional bonds sets the conditions for a partner-centered sexuality, above all among women, in which 'love' and 'fidelity' are central values." *If my own arguments are correct, Sigusch and Schmidt's comment that the workers* are following the patterns of the "liberal bourgeoisie" appears mildly ironical, for it was after all the workers who started the whole business off in the first place. "Lower-Class Sexuality: Some Emotional and Social Aspects in West German Males and Females," *Archives of Sexual Behavior,* 1 (1971): 29–44, quotation from 42. If Sigusch and Schmidt, H. L. Zetterberg, and other students of Europe are correct, an urgent problem for research is why American workers are so different. Sigusch and Schmidt drew upon Lee Rainwater for their characterization of working-class sexuality in the United States, especially upon his classic work *And the Poor Get Children: Sex, Contraception and Family Planning in the Working Class* (Chicago, 1960).

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companied wage labor. Peter Stearns has recently reviewed the German literature, finding toward the end of the nineteenth century (a period inconveniently late for the case I wish to present here) "recognition of greater independence for the woman. . . . There is suggested here a new sentiment within the family, the possibility of greater affection for the children, who were not underfoot all the time, and greater sensuality and equality in the relationship between man and wife." And Rudolf Braun, in his sensitive reconstruction of life among cottage and factory workers in the Zurich highlands, notes massive shifts in family patterns, starting with the eighteenth century. While Braun is silent on specific changes in the relationships between married men and women, he pulls back the canvas for a brief instant to reveal, for example, women forgetting how to cook. Why, Braun asks, were ready-made foods in such great demand in factory towns?

> It was not merely the pressure to eat at the workplace that accelerated the demand for prepared dishes, nor the lack of time at home, but also the woman factory worker's lack of skill in cooking. Bound to the machine and the factory since earliest childhood, she inadequately learned the arts of cooking and homemaking. We have seen these complaints since the woman cottage workers of the *ancien régime,* but with factory workers they become even more urgent.[16]

One can imagine that the authority patterns among traditional petit-bourgeois families were as different from those of worker couples out on the frontier of economic advance as night is from day.

Evidence is more abundant that young unmarried women were rebelling against parental and social authority in the period from 1750 to 1850. To draw upon my own research, I noted in early nineteenth-century Bavaria an absolute squall of outrage from middle-class observers of popular life, seated for the most part in lower levels of the governmental bureaucracy, about a new spirit of independence among young women in agricultural labor and domestic service.[17] Through this chorus of complaints ran the themes of escape and experimentation, of throwing off old superordinates and codes, and of, in general, what a much later generation of emancipators was to call "liberation."

There was the theme of escape from old jobs. Young women wanted, when possible, to forsake domestic service for employment that would safe-

[16] Neil Smelser, *Social Change in the Industrial Revolution: An Application of Theory to the British Cotton Industry* (Chicago, 1959), 209 and the chapter "Pressures on the Family Division of Labour," 180–224; Peter Stearns, "Adaptation to Industrialization: German Workers As a Test Case," *Central European History,* 3 (1970): 303–31, especially 307; Rudolf Braun, *Sozialer und Kultureller Wandel in einem ländlichen Industriegebiet (Zürcher Oberland) unter Einwirkung des Maschinen- und Fabrikwesens im 19. und 20. Jahrhundert* (Erlenbach-Zurich, 1965), 201. On the lack of domestic accomplishments among English working-class wives, see Margaret Hewitt, *Wives & Mothers in Victorian Industry* (London, 1958), ch. 6, "The Married Operative As a Home-Maker," 62–84.

[17] See Edward Shorter, "'*La Vie Intime*' Beiträge zu seiner Geschichte am Beispiel des kulturellen Wandels in den bayerischen Unterschichten im 19. Jahrhundert," *Kölner Zeitschrift für Soziologie,* 16 (1973): 530–49.

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guard personal independence. The unpopularity of service may be seen in the cries about a shortage of rural labor (*Dienstbotenmangel*) that became a constant theme in social criticism from the mid-eighteenth century onward.[18] Or, to take another sort of example, Munich's police chief noted in 1815:

> It is sad, and most difficult for the police to prevent, that so many young girls leave service when they grow tired of waiting on people and under one pretext or another take a room somewhere, living from their own industry. But they do little real work and let themselves be supported by boyfriends; they become pregnant and then are abandoned.[19]

And there was the theme of escape from old residences. Young women wanted to live alone, in their own quarters and away from the oppressive supervision of either parents or employers. In the late 1830s the indignant provincial government of Würzburg observed:

> In our province the so-called practice of *Eigenzimmern* is quite customary, according to which the deflorated daughter leaves the parental house and rents a room elsewhere, not necessarily to avoid the reproaches of the parents for her misdeeds, but in order to move more freely, to accommodate the visit of the boyfriend [*Zuhälter*] and with him to live in concubinage [*wilde Ehe*].[20]

On the matter of escape from old personal styles, let Joseph Maria Johann Nepomuck Freiherr von Frauenberg, archbishop of Bamberg, speak:

> A most detrimental alteration in the character of the female gender [has taken place]. Earlier, women distinguished themselves through their soft, withdrawn, modest, and chaste being, while nowadays they take part in all public entertainments, indeed providing some, set the tone [*den Ton angeben*], and so have entirely departed from their natural situation. Thus has female morality disappeared.

The archbishop noted this development had occurred principally in the cities. There were other complaints about how female servants and hired hands would squander their entire wages in buying expeditions to the cities, returning to the farm with clothes alien to native folkways. Still other laments were voiced about feminine indifference to pastoral authority and about newly grasping, calculating female attitudes to wage matters. All these threads led back in the opinion of contemporaries—and rightly so I think—to sexuality and thus ultimately to fertility: "In the countryside a young girl who has preserved her virgin purity until age twenty is

[18] See, for example, Hanns Platzer, *Geschichte der ländlichen Arbeitsverhältnisse in Bayern* (Munich, 1904).

[19] "Auszug aus dem Tags-Berichte des k. Polizei-Directors dahier . . . 2 August 1815. Betreffend: Die vielen ledigen Weibspersonen dahier," Bayerisches Hauptstaatsarchiv, Munich, MI 46545.

[20] Report from provincial government to interior ministry, "Das Verhältnis der unehelichen zu den ehelichen Geburten betr," Feb. 30, 1839, *ibid.*, MI 46556.

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exceptional, and moreover encounters even among her girlfriends no recognition."[21]

Perhaps Bavaria was not typical of the rest of Europe, though I believe that it was, for within its frontiers the kingdom harbored a remarkable diversity of social and economic arrangements. Perhaps, even more serious for the case I wish to make, male complaints about "moral breakdown" among young women reflected sooner the beholder's own libidinal preoccupations than a change in objective social conditions. Perhaps, too, nostalgia is close to being a historical constant, so that most men who search their own memories invariably see behind the outlines of a gray, disorganized present the golden harmony of an idealized past. Yet in this case I doubt it. And I suspect that future research will verify that this particular set of social critics at this particular point in time—the years 1800–40—were onto something. The objective order of the real world was in fact changing, and a shift in the position of women was moving the ground directly from under the feet of these "patriarchs."

THESE CHANGES in the mentalities and sexual comportment of women may ultimately be linked to a variety of changes in economic structure that one might summarize under the label "capitalism." Three salients of industrial advance mattered to fertility, and two of the three made more of a difference to women than to men. Figure 3 summarizes the argument.

First, capitalism meant the formation of a proletarian subculture. Large numbers of people who had in common the fact that they were wage laborers found themselves living together in the same communities. Because the material conditions of their lives differentiated them clearly from the surrounding social order of small proprietors, these newly aggregated workers in both agriculture and industry began to develop their own rules for doing cultural business, which is, after all, the essence of a subculture. A way of life specific to the working classes began to elaborate itself within the large farm areas of modernizing agriculture, upon the upland slopes where the putting out of textiles and nail manufacture was thriving, and within the newly blossoming industrial cities themselves.[22]

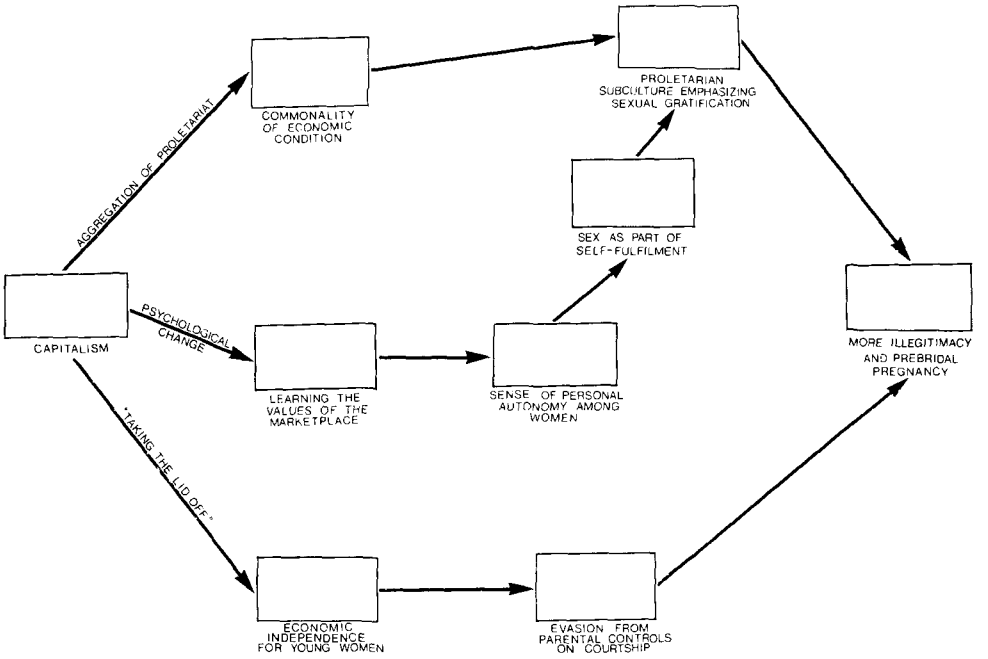
The subculture would sooner or later matter to fertility by providing alternative sets of rules for sexual comportment, target family sizes, and

[21] For the archbishop's statement, see petition to the king, Apr. 3, 1838, *ibid.*; the final quotation is from provincial government of the Obermainkreis, in "Auszüge aus den dreijährigen Verwaltungs-Berichte, 1830–33: Öffentliche Sitten," *ibid.*, MI 15396.

[22] Least known about the formation of an industrial labor force in Europe is still how cultural patterns changed. Among recent works, however, are Rudolf Braun, *Industrialisierung und Volksleben: Die Veränderungen der Lebensformen in einem ländlichen Industriegebiet vor 1800 (Zürcher Oberland)* (Erlenbach-Zurich, 1960), especially 90–154, 181–212; R. P. Neuman, "Industrialization and Sexual Behavior: Some Aspects of Working-Class Life in Imperial Germany," in Robert J. Bezucha, ed., *Modern European Social History* (Lexington, Mass., 1972), 270–98; and Jeffry Kaplow, "The Culture of Poverty in Paris on the Eve of the Revolution," *International Review of Social History*, 12 (1967): 277–91.

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FIG. 3. A MODEL LINKING CAPITALISM TO THE SEXUAL EMANCIPATION OF WOMEN



new techniques for contraception and abortion. But subcultures are especially important in the area of legitimation of behavior about which the individual might otherwise feel uneasy. It is now common knowledge that the charter culture of traditional Europe had internalized within young people a host of restraints against intercourse. So that if before 1750 there was relatively little premarital intercourse, it was not necessarily because external supervision was totalitarian in its strictness but because most people within the culture shared the belief that premarital sex was wrong. When in later years sex before marriage became commonplace, it was because a new generation of sexually active young men and women felt their behavior was socially accepted, at least by their peers. The point is that if an individual is going to bend the operating rules of the dominant culture, he must feel that members of his own group, whose good opinion he treasures, will support his venturesomeness.[23]

[23] Michael Schofield has recently pointed out the interaction between peer-group membership and teenage sexual activation in contemporary Britain: "One of the strongest influences on a teenager's behaviour in any sphere is the desire to be like other teenagers, and sexual activities are no exception. Experienced boys spend more time in teenage groups and seek advice from friends rather than parents. They are influenced by other teenagers and by the entertainment industry and the flourishing commercial market directed towards teenage spending. Conformity with other teenagers is very important." The same is roughly true of girls, except that, additionally, girls are more in revolt against their families than are boys and so see sex as part of a domestic power struggle. Thus Schofield finds sex-specific differences precisely where my argument, based on historical material, would have predicted them: membership in a subculture is important for both sexes alike, but individual self-awareness and "taking off the lid" in revolt against the parents affected young women more than young men. See *The Sexual Behaviour of Young People* (Harmondsworth, Middlesex, 1968), 80, 133, 193–95, and especially 202–08; quotation from 204.

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The proletarian subculture was, of course, indulgent of eroticism. Yet this particular indulgence must not be attributed without further argument to the industrial origins of the subculture. The fact that a subculture exists does not automatically mean that its specific operating rules must be libertine. Indeed many subcultures with quite repressive sexual values have flourished in the past, such as the colonies of nineteenth-century pietists in the United States. Some additional aspect of industrialism must therefore be adduced to explain the expressly permissive sexual content of the European proletarian subculture.

The second important dimension of capitalism lay in the mentality of the market place. In the eighteenth and early nineteenth centuries the market economy encroached steadily at the cost of the moral economy, and the values of individual self-interest and competitiveness that people learned in the market were soon transferred to other areas of life. It was this process of the transfer of values that gave the proletarian subculture its libertine moral caste.

The years after 1750 saw the intrusion of the principles of the market place into popular life. In early modern Europe trade in foodstuffs and in most nonagricultural products was tightly regulated by communal and corporate bodies, so that the Continent was fragmented into countless tiny local markets, kept through a complot of regulation and poor transportation as hermetically sealed compartments. Of course long-distance trade existed, yet most of the labor force was involved in local production along noncapitalistic lines. German political economists made a classic distinction between *Export-* and *Lokalgewerbe,* and most of the population lived from the latter. Then late in the eighteenth century these locally administered economies began to be engulfed by free markets of vast territorial scope. The struggle over free trade in grain in France has been often told; the losing battle of German guilds against pack pedlars, retail merchandise shops, unlicensed competitors, and the Customs Union is similarly familiar. Everywhere the moral economy regulated by the village fathers lost out to free competition regulated only by the invisible hand of the price mechanism.[24]

Contact with these new labor markets was the most direct source of personal autonomy. As women became immersed in the market, they

[24] The literature on how the free market conquered the moral economy is enormous. Among recent work may be mentioned Louise A. Tilly, "The Food Riot As a Form of Political Conflict in France," *Journal of Interdisciplinary History,* 2 (1971): 23–57; Francois Furet, "Le catéchisme révolutionnaire," *Annales: Économies, Sociétés, Civilisations,* 26 (1971): 255–89, especially 265–66; E. J. Hobsbawm and George Rudé, *Captain Swing* (New York, 1968), especially pt. 1; Wilhelm Abel *et al., Handwerksgeschichte in neuer Sicht* (Göttingen, 1970), especially the contribution by Friedrich-Wilhelm Henning, "Die Einführung der Gewerbefreiheit und ihre Auswirkungen auf das Handwerk in Deutschland," 142–72. Mack Walker has traced the disappearance of a whole way of life, once incubated in the hermetically sealed small German town, in *German Home Towns: Community, State, and General Estate, 1648–1871* (Ithaca, 1971), especially 108–42, 405–31.

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learned its values. I have elsewhere suggested that capitalism's mental habits of maximizing one's self-interest and sacrificing community goals to individual profit transfer easily to other thought processes.[25] It seems a plausible proposition that people assimilate in the market place an integrated, coherent set of values about social behavior and personal independence and that these values quickly inform the noneconomic realms of individual mentalities. If this logic holds true, we may identify exposure to the market place as a prime source of female emancipation, for women who learned autonomy and maximization of self-interest in the economy would quickly stumble upon these concepts within the family as well. Men would also have learned these values, but then it was men who had traditionally been the dominant sex; a more sensitive attunement to questions of individuality left men, if anything, less able to defend themselves against the demands for autonomy of their wives and daughters. The moral authority of traditional society was of a piece; the same communitarian principles that held together the moral economy also maintained the authoritarian family. And they crumbled together as well.

Thus a second crucial consequence of capitalism for women came in the area of personal values: an unwillingness to accept the dictates of superordinates and a new readiness to experiment with personal freedom and gratification. The reader should at this point bear in mind that we have to juggle simultaneously three different effects of capitalism:[26] the first dimension of subculture weakened traditional moral taboos and destroyed internalized antisexual values; the second dimension, which we have just considered, quickened interest in intercourse as an aspect of personality development; and the third dimension of capitalism, to which we now turn, removed many of the external controls upon female sexual emancipation.

This last principal salient of industrial advance worked in the interest of women by modifying with wage labor the balance of power in the family. Paid employment meant that women would bring a distinct, quantifiable contribution to the family's resources, and accordingly would probably be entitled to a greater voice in the disposal of these resources. As many sociologists of the family have noted, the wife's (or daughter's) influence

[25] This argument has been presented in greater detail in Shorter, "Sexual Revolution, Social Change and Illegitimacy." An important work linking social change to the search for personal autonomy (for men) is Fred Weinstein and Gerald M. Platt, *The Wish to be Free: Society, Psyche, and Value Change* (Berkeley, 1969).

[26] It seems a bit unfair to make capitalism carry the brunt of the analysis, in view of the fact that two of the three dimensions of industrial advance affecting sexual behavior would also occur under socialism. The agglomeration of a proletarian work force is an accompaniment of any form of modern industry, as is the increased leverage within family circles women get through participation in the labor force. Only the noneconomic personal values that people learned from the logic of the market place—that impersonal, "ruthless" device for allocating resources and mobilizing capital—would not necessarily be found under socialism. Yet European economic development took place under the aegis of capitalism, not socialism, and the phrase appears just.

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within the conjugal unit is a direct function of the status she enjoys in the outside world and of the resources she is able to import from that world into the family circle. Richard F. Tomasson has convincingly explained the historical development and the present-day international singularity of the Swedish family with such an approach, arguing also that, "Where females have greater equality and are subject to less occupational and social differentiation, the premarital sex codes will be more permissive than where the female's status is completely or primarily dependent on the status of her husband."[27] Altogether, capitalism entailed a quite material source of female independence and autonomy, increasing vastly the leverage formerly obtained from customary, dependent, unpaid, "women's work."

Popular involvement in the market economy started with the young and the poor and ended with the older and more prosperous. It was the most marginal whom capitalism could first detach from their traditional economic moorings, and so in the eighteenth century the young members of the proletarian classes that population growth had been creating went first to the cottage looms and spinning wheels. Thereafter ever more prosperous groups of the traditional economy found themselves pulled into the flux of the market, so that by the late nineteenth century even the most isolated sectors of the old middle class had been plunged into price competition and profit rationality. Immersion in the market progressed by stages.

Early in the eighteenth century the putting-out system began its conquest of the countryside, drawing in the landless poor. Then, in the course of the century agricultural capitalism began to encroach upon traditional subsistence and manorial farming, recruiting from among the landless and especially from the youth, for often unmarried laborers would live in the farmer's house, or newly married couples in nearby cottages. Next came migration to the newly rising factories and mills. The timing varied from one region to another, but normally it was the youth whom the fresh modern sector pulled from small farms and craft shops into factories.

In the nineteenth century industrial growth created a prosperous new middle class of administrators and clerks, of technicians and professionals. Because these people had often to endure long delays before marriage, women entered their childbearing periods at relatively advanced ages

[27] Richard F. Tomasson, *Sweden: Prototype of Modern Society* (New York, 1970), 165–98, especially 180. In modern European agriculture, of course, the labor of peasant women is essential to the profit of the farm, and even though they do not have salaried jobs, such women supply vital resources to the family's economy. See, for example, Frederik Barth, "Family Life in a Central Norwegian Mountain Community," in T. D. Eliot and A. Nillman, eds., *Norway's Families: Trends, Problems, Programs* (Philadelphia, 1960), 81–107, on the equality of the contemporary peasant woman within the household. This "modern" pattern in Norway dates back to at least the 1850s and 1860s, when Eilert Sundt noted that the woman's contribution to farm labor might equal the man's. Sundt's writings are reviewed in Michael Drake, *Population and Society in Norway, 1735–1865* (Cambridge, 1969), 145. Drake, however, believes this Norwegian pattern of female labor input to have been unusual in Western Europe at that time.

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and largely abstained from intercourse beforehand. Finally, in the nineteenth century capitalism tore at the heart of the traditional old middle class itself, rather than merely at the supernumary poor. Across the Continent the masters of craft shops had to accommodate themselves to industrial capitalism, either by servicing the new factories or by going to work in them. And the depopulation of the countryside on the threshold of the twentieth century is an oft-told tale. It was frequently as mature men and women that these families were forced out of the traditional sector, of which they had constituted the backbone.

Thus the market started with the youngest and lowliest on the age-status spectrum and concluded with the most established and mature. It was also in this order that, I suggest, the spirit of female emancipation spread, from young and poor to well to do and middle-aged.

How, PRECISELY, did these massive shifts in economic structure, culture, and individual mentalities affect either marital or nonmarital fertility? The linkages between emancipation and the increase in illegitimacy seem crisp and strong; those between capitalism and marital fertility are largely artifacts.

For the unmarried woman capitalism meant personal freedom, which meant in turn sexual freedom. The young woman could withstand parental sanctions against her sexual and emotional independence because the modern sector promised employment, economic self-sufficiency, and if need be, migration from home to another town. Such independence meant often, as we have seen, a paramour and therewith, in the absence of birth control, illegitimacy.[28]

[28] Aggregate illegitimacy rates provide one way to test this hypothesis. If the modern sector promised greater freedom for women, one would expect a higher propensity to illegitimacy among, say, factory women than among women in occupations likely to be tied to middle-class life and family controls, such as small-shop merchandising. Yet such published data are rare, and what studies exist lead to divergent conclusions. Auguste Lange, for example, found that in Baden during the 1890s female factory workers had easily the highest propensity to illegitimacy, followed by domestic servants, and then, at a very distant remove, by women in agriculture and such genteel occupations as schoolteaching and nursing, results consistent with my argument. *Die unehelichen Geburten in Baden: Eine Untersuchung über ihre Bedingungen und ihre Entwicklung* (Karlsruhe, 1912), 96*–98*. L. Berger, on the other hand, determined that in Prussia in 1907 domestic servants had the highest illegitimate fertility, followed closely by factory workers and peasants (who tied for second place once age is controlled for), and then far behind by commerce, the free professions, and women without an occupation. "Untersuchungen über den Zusammenhang zwischen Beruf und Fruchtbarkeit under besonderer Berücksichtigung des Königreichs Preussen," *Zeitschrift des königlich preussischen statistischen Landesamts*, 52 (1912): 225–58, especially 230–31. Of three French textile towns in 1840, factory women in Châlons-sur-Marne and Reims represented no higher a proportion of illegitimate mothers than they did of the population as a whole. Only in Troyes did factory women contribute considerably more of the illegitimacy than they did of the city's population. In 1840, fifty-three per cent of all illegitimate births came from the "classe manufacturière," a class that represented only one-third of the city's population. Ernest Bertrand, "Essai sur la moralité des classes ouvrières dans leur vie privée," *Journal de la société de statistique de Paris*, 13 (1872): 86–95.

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Now, as stated, it seems likely that capitalism affected considerably the mentalities of first young and then of progressively older married women, but to the extent that it influenced their fertility we are dealing with a statistical trick. Or perhaps the relationship is partly artifactual, partly genuine. There are two possible linkages between the increase in the fertility of young married women and economic change, one relatively banal and straightforward, the other more speculative but, if right, the key to an important aspect of family history.

Marital fertility rose for some ages first of all because of the increase in bridal pregnancy (see appendix 2 for prenuptial-conception data.) Fertility rates for the age group in which women marry will go up if many women are pregnant as they enter that group. Thus a premarital conception might mean that the number of children born to an average woman in the 25–29 year-old group (who married at age 25) would be three rather than two.

General fertility rises also in consequence of prebridal pregnancy, assuming that birth control is not being practiced. Women pregnant outside of wedlock have a head start of at least one child on their nonpregnant peers. And indeed such women might take the lead by several children, for those pregnant prebridally probably married earlier than those not pregnant and so would have more highly fecund stretches of married life ahead of them: in the absence of birth control additional conceptions would doubtless take place. Age at marriage has always functioned as the gate keeper of family size in Europe, and so it is no surprise that those forced to marry early ended up with larger families. If a substantial minority of women become involved in prebridal pregnancy, general fertility will rise significantly without the median age at marriage dropping much at all.[29]

Yet prebridal pregnancy does not appear to be the whole story in rising marital fertility. Even after local time series have been expressly standardized for prenuptial conceptions, some residual fertility increase will surely remain. Even after out-of-wedlock conceptions have been discounted, we are likely to continue to encounter an updrift in the birth rates of women in their twenties. Several possible explanations may be brought to bear on this: perhaps an abandonment of breast feeding, such as John Knodel discovered in nineteenth-century Anhausen (Bavaria) was the cause, so that married women became fertile again more quickly, or resumed sexual relations more rapidly, after the birth of a previous child;[30] or one might

[29] Daniel Smith has written me in a letter that, if we assume the average marriage age of nonpregnant women to be 28 and that of bridally pregnant women to be 25, an increase over time in the proportion of pregnant brides from 20 to 40 per cent would reduce the mean age at marriage by only .6 years, from 27.4 to 26.8. As may be seen from appendix 2, such an increase in prebridal pregnancy was quite a common occurrence.

[30] John Knodel, "Two and a Half Centuries of Demographic History in a Bavarian Village," *Population Studies*, 24 (1970): 353–76. See also John Knodel and Etienne van de Walle, "Breast

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speculate about lower rates of involuntary foetal loss, for whatever reason. I prefer, however, to call attention to the intervening variable of intercourse.

The possibility that family sizes rose after 1750 as a perverse, ironical consequence of female emancipation seems at least worth investigating further, via the following logic: women with a new sense of personal autonomy found themselves in companionate marriages much more often than did women with traditional mentalities; in such marriages the frequency of intercourse was higher than for traditional marriages, simply because women in them experienced sex as a pleasurable act; in the absence of birth control, more intercourse led to more pregnancies and therewith to increased marital fertility.

Will this fragile chain of reasoning bear even the weight of logical inspection, to say nothing of the evidence? In appraising the relationship among styles of family life, the condition of women, and frequency of intercourse, let us remind ourselves of three "probable facts," that is, pieces of evidence that have been nailed into place with some firmness in the literature, yet are not indisputable.

First, it is likely that women in traditional society did not enjoy intercourse, that they attempted to evade their husbands' sexual demands, although women probably were willing to make themselves available simply as a matter of wifely duty. Just as men held the political and economic power in traditional society, they possessed the sexual controls as well. And if Helmut Möller is right, husbands exercised their sexual perquisites so peremptorily and brutally as to leave women little erotic pleasure:

> [X], who showed neither liking nor affection for the older woman who had been forced upon him as wife, nonetheless prided himself in his later biography [1798] as having produced ten children with her in fourteen years. I do not have the impression that such continuous pregnancies were motivated by the Biblical commandment to multiply oneself, nor from a corresponding aversion to contraceptive devices. Much more we see here at work that structure of household status that demanded that the wife do in everything her husband's bidding and that thus permitted him to satisfy his sexual desires even at the possible cost of the physical strain of the wife. Given the low level of masculine erotic accomplishment, the husband must be blamed for abruptness in gratifying his lusts, leaving the wife much more dependent upon the pleasures of fore- and afterplay [which Möller makes clear were virtually nonexistent, given the genital orientation of petit-bourgeois husbands].[31]

If the patterns Möller reports were in fact widespread, it is difficult to see how intercourse could have been anything other than a tiresome obligation for most women in traditional families.

Feeding, Fertility and Infant Mortality: An Analysis of Some Early German Data," *ibid.*, 21 (1967): 109–31.

[31] Möller, *Die kleinbürgerliche Familie im 18. Jahrhundert*, 287–88.

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Second, it is probable that the eighteenth century saw the initial appearance of the companionate marriage among the popular classes. Philippe Ariès, of course, has established that in the early modern period deep affective attachments between spouses, articulated as romantic love, were found increasingly among the upper classes. It is also possible that along a spectrum of workaday life, from butchers, bakers, and candlestick makers to field hands and journeyman weavers, such romantic sentiments replaced "institutional" ties in cementing husbands and wives together. Rudolf Braun has spotted among the textile population in the Zurich highlands "a new function of marriage." "People no longer enter marriages (or renounce entering them) as in peasant circles for the sake of the farm, but because they see in marriage the fulfillment of individual happiness. Marriage becomes *Paarehe*." And Jean-Louis Flandrin considers companionate marriage a powerful new social force among the pious of eighteenth-century France, so that a married couple acted as though they were in an extramarital liaison. "The new development was that husbands behaved with wives as with mistresses, and that the spouse 'acted towards her husband as though with a lover.' Thus the true faith consisted suddenly in believing that marriage is *une relation amoureuse*, with the legitimation of sacrament."[32]

Among traditional families, by way of contrast, affect and sentiment were much less important in marital relations than the need to maintain the family's efficiency in producing goods and its integrity in transmitting property and status from one generation to the next. These were institutional roles that a demanding community had thrust upon the complaisant backs of the conjugal pair. In companionate marriage individual self-fulfillment through the means of romantic love came first.

The third assertion is that frequency of intercourse exerts some influence upon fertility. Of course, no intercourse no conception. But what difference higher ranges of coital frequencies make in the likelihood of conception is still something of a puzzle to demographers, given the absence of reliable information on the number of times during a menstrual cycle the typical couple will have coitus. But it is mathematically evident

[32] Philippe Ariès, *Centuries of Childhood: A Social History of Family Life*, tr. Robert Baldick (London, 1962); Braun, *Industrialisierung und Volksleben*, 72; Jean-Louis Flandrin, "Contraception, mariage et relations amoureuses dans l'Occident chrétien," *Annales: Économies, Sociétés, Civilisations*, 24 (1969): 1370–90, especially 1390. I am sympathetic to Flandrin's argument that in the eighteenth century a new style of marriage based upon romantic love began to emerge, but I question his assertion that married couples borrowed from premarital fornicators the lore of contraception. I believe the low illegitimacy ratios of the period before 1750 to be the result of abstinence rather than of contraception. If Flandrin were right, one would expect contraception within marriage to have begun among young lower-class newlyweds rather than among mature bourgeois families, for the premarital lovers would surely have taken their contraceptive techniques with them into marriage. For a case study—closer, however, to caricature than to ideal type—of conjugal families in North America with newly autonomous women, see Richard Sennett, *Families against the City: Middle Class Homes of Industrial Chicago, 1872–1890* (Cambridge, Mass., 1970).

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that an increase in the rate of intercourse will result in an increase, although of smaller proportions, in the likelihood of conception.[33] Historically, coital frequencies seem to have affected fertility levels in at least one documented instance: eighteenth-century New England. Daniel Smith attributes the tendency for women with older husbands to be less fertile than women with equal-age husbands to an intercourse variable: older men are less active sexually than younger men. Thus, according to Smith, "The lower coital frequency in marriages with older husbands has a substantial effect on the fertility of wives over age thirty."[34] If lower incidences of intercourse contributed to a reduction of fertility in Hingham, Massachusetts, over the long run, might higher frequencies of coitus not similarly have elevated marital fertility in eighteenth-century Europe?

These "modern" marriages, which Jean-Louis Flandrin and Rudolf Braun describe, probably experienced higher rates of intercourse than did traditional marriages. Romantic love means mutual sexual gratification and ego autonomy for both partners. Men may safely be presumed always to have been avid for intercourse, so it was in the area of women's sexual needs and gratifications that companionate marriage would matter. If coitus had become an aspect of personal fulfillment, instead of an irksome duty, we may assume that women in these new-style families would actively encourage intercourse, rather than reluctantly submitting from time to time to their husband's demands.

If this chain of speculative reasoning is right, several factors that have hitherto lurked in the shadows must be given careful examination before eighteenth-century fertility movements can be explained. Most important is the question of family values and styles of marriage. For among those families where traditional models of behavior prevailed between husbands and wives, a growing surge of female emancipation would probably have caused a reduction in coital frequencies, as women became ever less willing to do the sexual bidding of men whose all-power they found ever less legitimate. It is within "modern" companionate marriages that women's emancipation may well have caused an increase in inter-

[33] Peter A. Lachenbruch has shown, assuming a fairly short fertile period and a uniform distribution of coital experience throughout a typical menstrual cycle, that an increase in coital frequencies per cycle of 8 to 12 will increase the probability of conception from .23 to .32. "Frequency and Timing of Intercourse: Its Relation to the Probability of Conception," *Population Studies*, 21 (1967): 23–31. Research by John C. Barrett and John Marshall demonstrates an even closer relationship between the frequency of intercourse and the likelihood of conception, so that an increase in coitus from every sixth day to every third nearly doubles the chances of conception. The authors conclude: "The well-recognized differences in the fecundability of women (indicated, for example, by the nature of variations in family size in populations in which little or no contraception is practiced) may therefore be largely explicable on the basis of differences in the patterns of coital behavior, which now acquire greater significance." "The Risk of Conception on Different Days of the Menstrual Cycle," *ibid.*, 23 (1969): 455–61; quotation from 461.

[34] Daniel Smith, "The Demographic History of Colonial New England," *Journal of Economic History*, 32 (1972): 165–83, especially 180–81.

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course. And the possibility that these higher coital frequencies might have influenced fertility should at least be closely researched before being rejected.

CONTRACEPTION AND ABORTION traveled in exactly the reverse direction from female emancipation. Deliberate family limitation began among the older women of the old middle class in the eighteenth century and traveled downward along the age-status spectrum to reach young proletarian women only late in the nineteenth. Recent research permits us to mark some of the milestones of this progression.

Late in the seventeenth century upper-class families among Geneva's bourgeoisie were practicing birth control, the older women beginning first. It was the solid middle classes in eighteenth-century France who first started curbing family size, with birth rates among older women falling faster than among younger. Clearly we are dealing with substantial propertied citizens who, after attaining a modest family size, decided to call a halt. Excellent nineteenth-century data on Scandinavian marital fertility show the birth rates of the oldest women falling most rapidly, with the effect that the lower the age of the mother, the slower the pace of the fertility decline. German and English data indicate that the higher the social and economic class of the family, the more rapid was the slide in its fertility during the nineteenth century.[35]

Thus the demographic evidence points to the "reverse diagonal" in the spread of birth control. As is suggested in figure 2, the movement began among mature married women of sober middle-class families and spread with the passage of time to younger women of lower status. Women's emancipation moved, of course, in precisely the opposite direction.[36]

[35] For Geneva, see Louis Henry, *Anciennes familles genevoises: Etude démographique, XVe–XXe siècles* (Paris, 1956), 75–81. See the eighteenth-century studies cited in appendix 1. Louis Henry and Philippe Ariès have both stressed prudential elements in the decisions of bourgeois families to begin birth control. For their essays, Henry, "L'apport des témoignages et de la statistique," and Ariès, "Interpretation pour une histoire des mentalités," both in Hélène Bergues, ed., *La Prévention des naissances dans la famille: Ses origines dans les temps modernes* (Paris, 1960), 361–76, 311–27, respectively. On Denmark, P. C. Matthiessen's recent monograph should be consulted, *Some Aspects of the Demographic Transition in Denmark* (Copenhagen, 1970), *passim*. On Norway, *Historisk Statistikk, 1968*, 29. On Sweden, *Historisk statistik för Sverige*, vol. 1, p. 106. John Knodel has reviewed the German evidence in his forthcoming monograph, *The Decline of German Fertility;* on England, see John W. Innes, *Class Fertility Trends in England and Wales, 1876–1934* (Princeton, 1938).

[36] The Bankses, who argue in *Feminism and Family Planning* that the woman's movement in England did not set off the family limitation movement, attribute increased contraception to growing status ambitions among middle-class males. The Bankses suggest that if the burghers' wives were to be genteel, their families would have to be small because the image of a "perfect lady" was difficult to reconcile with hordes of squalling brats. If the argument in this paper is correct, the Bankses should look as much to the lower classes as to the middle for the social sources of female autonomy. Combining the Banks and Shorter hypotheses, it would appear that the Bradlaugh-Besant trial of 1877 rapidly diffused news of birth-control practices, confined until then to decidedly unliberated upper-middle-class families, among the eager but ignorant popular classes.

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The reasons for the spread of birth-control practices have been much debated. One group has suggested that structural changes in society were paramount, that birth control was an adjustment to new social and economic circumstances, a response to modernization on the part of couples whose changed situations did not permit the customarily large families. The other group, drawing strength from the simultaneity of the decline in the birth rate, argues that birth-control lore diffused quickly to all women in the population, regardless of their social or economic status or their place of residence. We have, thus, the adjustment versus the diffusion (sometimes called the "innovation") points of view.[37]

If the argument presented in this paper is right, the two views should be combined. There were probably two phases in the spread of birth control among European women. In the first phase birth control extended among middle-class women only. To go by recent French research, family limitation began in the economically backward, depressed regions of the land, where population had been growing rapidly. The first Malthusian families were those with the most to lose from distress: the traditional middle classes in commerce and agriculture.[38] Contraception was, among these strata, a defensive reaction to the threat of modernity: cutting one's vulnerability to impoverishment by reducing one's family size. Between perhaps 1750 and 1850 the spread of birth control followed the shadow side of industrialization, moving into those pockets of hardship and anxiety that economic and demographic growth were leaving in their wake. Thus during this time, and among these strata of the population, birth control was introduced through adjustment, not through diffusion. In the second phase birth-control lore leaped the gap from the old middle class to the new working classes. The mass media, in giving publicity to the Bradlaugh-

[37] The classic diffusionist statement is Norman E. Himes, *Medical History of Contraception* (London, 1936; reprint, New York, 1963), pt. 5, especially ch. 10. Among contemporary researchers Gösta Carlsson inclines toward the adjustment school, Etienne van de Walle toward the "diffusionist" or "innovationist." Both recognize the complexity of the problem, and battle lines are not sharply drawn. Van de Walle, "Déclin de fécondité"; Carlsson, "The Decline of Fertility: Innovation or Adjustment Process," *Population Studies*, 20 (1966): 149–74. The "dip-crash" effect that many fertility curves demonstrate—i.e., an initial hesitant downward movement followed by a drastic plunge—may reflect this two-stage spread. In phase 1 relatively few families are affected, and their fertility control comes slowly as structural changes in the economy move slowly over them. So fertility aggregates decline only modestly. In phase 2 diffusion comes rapidly to the masses of the population, for the length of time required for the good news of birth control to spread is brief. And so aggregates fall off dramatically. This "dip-crash" curve, when specifically attributable to birth control, appears in a number of aggregate series. See, for example, Jacques Dupâquier and Marcel Lachiver, "Sur les débuts de la contraception en France ou les deux malthusianismes," *Annales: Économies, Sociétés, Civilisations*, 24 (1969): 1391–1406; Gösta Carlsson, "Nineteenth-Century Fertility Oscillations," *Population Studies*, 24 (1970): 413–22.

[38] Pierre Chaunu writes, summing up the corpus of French local demographic studies: "[Contraception] moved in at the top of the social hierarchy in the cities and, with few exceptions, into the most miserable, impoverished, and retarded areas of the countryside. Malthusianism, often represented as the victory of man over man, appears much more in the eighteenth century as stigmata of failure and collective retardation." *La Civilisation de l'Europe classique* (Paris, 1970), 240.

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Besant trial, for example, caused birth-control awareness to burst suddenly into the consciousness of lower-class women, women who were avid to curb their family sizes as part and parcel of their drive for liberation but until then had not known quite how to go about it. A previous century of female emancipation had created a large lower-class population of women who were mentally prepared for small families, who desired ardently to curb their own fertility, but who until the second phase had lacked the requisite sophistication about reproductive biology. The spread of birth control during this phase resulted, therefore, from a process of diffusion rather than from adjustment. These women had already adjusted to changing structural conditions at the time they acquired liberated sentiments.

As long as birth control remained confined to the less numerous middle classes, as during phase one, overall fertility would remain high, or decline only slightly. But once, as in phase two, contraceptive and abortive knowledge diffused to the masses, fertility would plunge. And so late in the nineteenth century the parabola turned downward.

Because the predominant form of contraception in these years was doubtless coitus interruptus,[39] two corollaries of the spread of birth control cry out for further investigation: the role of abortion and the role of husband-wife interaction in the decision to practice birth control.

New-style women who were still locked into old-style families would have been helpless to impede their own fecundity as long as the available contraceptive techniques were male-initiated. It seems reasonable, therefore, to argue that the evident increase in abortion in the late nineteenth century came from autonomous women who, unable to inhibit conception because of the indifference or hostility of their husbands, had decided to destroy the fetus.[40] Accordingly, it may have been just as much abortifacient as contraceptive techniques that phase two diffused.

Within companionate marriages, an emancipated woman might successfully persuade her husband to begin coitus interruptus. The historian envisions in his mind's eye consultations over the supper table about target family size, the wife's fears of excess fertility, her reports of other women's conversations and decisions, the couple's pooling of wisdom on

[39] On the great popularity of coitus interruptus even in contemporary Europe, see D. V. Glass, "Family Limitation in Europe: A Survey of Recent Studies," in C. V. Kiser, ed., *Research in Family Planning* (Princeton, 1962), 231–61.

[40] A survey conducted around 1910 by Max Marcuse, a Berlin doctor, illustrates the extent of abortion. Among one hundred women interviewed, twenty-four had aborted themselves at least once. Only half of the conceptions among the one hundred resulted in children who survived until age one. "Zur Frage der Verbreitung und Methodik der willkürlichen Geburtenbeschränkung in Berliner Proletarierkreisen," *Sexual-Probleme, Zeitschrift für Sexualwissenschaft und Sexualpolitik*, 9 (1913): 752–80. On change over time in abortion, see, for example, Jean Sutter's discussion of the spread of "auto-avortement" toward the end of the nineteenth century. Sutter emphasizes advances in knowledge and technology in making possible the mass diffusion of abortive practices. "Sur la diffusion des méthodes contraceptives," in Bergues, *La Prévention des naissances dans la famille*, 346–48.

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how not to get pregnant, their joint decision to commence family limitation. Contemporary sociological research has emphasized the importance of communication between spouses, of open discussion of such intimate matters as birth control within the family circle. Where affectionate give-and-take is accepted, the chances are vastly improved that the husband will agree to constrain his sexual convenience and pleasure in the wife's interests.[41] Within traditional marriages such discussions are tabooed, and custom persuades the man of the continuing legitimacy of his sexual prerogatives.

IN THE LATE NINETEENTH CENTURY the curves of female emancipation and birth-control sophistication finally crossed, so that emancipated lower-class women finally got a means to curb fertility, and Malthusian middle-class women finally got a personal rationale for small families. Thus birth control entered the service of female emancipation, and the two forces, bound together, caused the fertility decline. Women's emancipation and birth control had started out as separate movements, drawing on distinct clienteles at opposite ends of the age-status spectrum. The history of family life and demographic change in the nineteenth century may be written as the history of their convergence.

Or at least, so it seems. These are plausible hypotheses rather than proven facts. And other scholars may find such speculations useful if only because they suggest an agenda for future research. If my argument is right, several areas of knowledge acquire a critical importance. First, we need to know more about changes in the structure and authority patterns of lower-class families during the period from 1750 to 1850. Did proletarian women in fact gradually attain a new awareness of their own autonomy, in the face of parental, marital, and community pressures for continuing dependence and subordination? And what structural changes, other than or in place of exposure to the market place, determined this shift in mentalities?

Second, the diffusion of birth-control knowledge requires investigation. Did information on how to prevent conception and how to abort oneself in fact travel from older women to younger, from higher classes to lower? What birth control techniques were used? And by what mechanisms did information about them travel: by word of mouth among friends, by the press, or by urban-rural networks of relatives visiting one another? The possibility must be ruled out that the popular classes had always had a store of contraceptive lore to draw upon but had merely decided not to raid this cultural larder until the force of events compelled them to do so.

[41] On equality of decision making and agreement on goals as factors in the adoption of contraception, see Andrée Michel, "Interaction and Family Planning in the French Urban Family," *Demography*, 4 (1967): 615–25. For other literature on this question, see Geoffrey Hawthorn's summary, *The Sociology of Fertility* (London, 1970), 91–96.

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Finally, the suffragist and birth-control movements demand further study. Was, as my argument indicates, the politicized feminist movement of the late nineteenth century the final phase in a process of emancipation that had begun a hundred years earlier among lower-class women? Or did it arise from a constellation of political events, such as the antislavery campaign, essentially unrelated to changes in family structure?

Yet the point of this article is not merely to raise questions. Still less is it to offer firm explanations of a subject as shrouded in darkness as popular patterns of sexuality and family life. It is rather to suggest that the central problem of modern European historical demography has been conceived too narrowly. And because of this confinement of vision, little progress has been made to date on the conventional problem of why marital fertility declined late in the nineteenth century. I am saying that this particular issue may most easily be resolved by seeing it as part of a larger integral historical movement: the parabolic curve of fertility. I am further arguing that answers to this grander problem should be sought in changes in the structure of the family, especially in the attitudes and situations of women, those members of the family who actually bear and care for children.

APPENDIX 1:
Sources for Age-Specific Marital Fertility Time Series in the Eighteenth and Early Nineteenth Centuries

Dates refer to years in which women married. Only those time series have been selected that touch at some point the last half of the eighteenth century.

ENGLAND: E. A. Wrigley's study of Colyton (Devon) has provided fresh evidence of dramatic increases in the marital fertility of women in most age brackets between the mid-seventeenth and mid-nineteenth centuries. "Family Limitation in Pre-Industrial England," *Economic History Review*, 2d ser., 19 (1966): 82–110, especially 89. Other English writings as well, notably those of J. T. Krause and of Phyllis Deane and W. A. Cole, had identified an eighteenth-century rise. Yet M. W. Flinn discounts the findings of this latter group as based on the faulty Parish Register Abstracts, unreliable because of incompletely registered births. *British Population Growth, 1700–1850* (London, 1970), 24–37.

SWEDEN: In Sweden the years 1771 to 1800 saw abrupt rises in the fertility of younger women, less or no change in that of older women. This is the period in which the Swedish illegitimacy rate commenced its first upward leap. For age-specific Swedish fertility data beginning in 1750, see Gustav Sundbärg, *Bevölkerungsstatistik Schwedens, 1750–1900,* reprinted as no. 3 of *Urval: Skriftserie utgiven av statistiska centralbyrån* (Stockholm, 1970), 117, 121.

NORTH AMERICA: Daniel Smith has found in Hingham, Massachusetts, sizeable increases in marital fertility at all age levels over the years 1691–1715 to 1721–80. See "The Demographic History of Colonial New England," *Journal of Economic History*, 32 (1972): 165–83, especially 178.

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GERMANY: Anhausen (Bavaria). John Knodel, "Two and a Half Centuries of Demographic History in a Bavarian Village," *Population Studies,* 24 (1970): 353–76, especially 369. Women married in 1800–99 have at all age levels beneath 39 higher fertility than those who married in 1692–1799.

Boitin (Mecklenburg). Jacques Houdaille, "Quelques résultats sur la démographie de trois villages d'Allemagne de 1750 à 1879," *Population,* 25 (1970): 649–54. Among the marriage cohorts of 1740–1809, 1810–39, and 1840–69, fertility increases for 20–24 through 35–39 year-olds. (Data for 20–24 group stop in 1840; rates for 25–29 group decrease from first period to second.)

Degerloch (Württemberg). Arthur Borghese, a graduate student at the University of Toronto, found in *Kirchenbücher* suggested by Professor David Sabean a strong upturn in the fertility of age groups 20–24, 25–29, and 30–34 from the marriage cohorts of 1800–09 to those of 1820–29. "Population Patterns in Degerloch, Württemberg, in the Eighteenth and Nineteenth Centuries."

Göttelfingen (Württemberg). Ilse Müller, "Bevölkerungsgeschichtliche Untersuchungen in drei Gemeinden des württembergischen Schwarzwaldes," *Archiv für Bevölkerungswissenschaft und Bevölkerungspolitik,* 9 (1939): 185–206, 247–64, especially 257. The average number of children born to women married before 20 and between 21 and 24 increased substantially from 1750–99 to 1800–49; average children born to women married at age 25–30 increased from 4.2 in 1676–99 to 6.7 in 1750–99, thereafter declining. Although not age-specific marital fertility rates, these figures seem nonetheless suggestive.

Remmesweiler (Saar). Jacques Houdaille, "La Population de Remmesweiler en Sarre aux XVIII^{e} et XIX^{e} siècles," *Population,* 25 (1970): 1183–91. Fertility increases among most young Protestant and Catholic age groups from 1780–1809 through to 1840–69, and sometimes to 1870–99.

Volkshardinghausen (Hesse). Jacques Houdaille, "Quelques résultats sur la démographie de trois villages d'Allemagne." Rise for 20–24 year-old women from "avant 1810" to 1869. In this article Houdaille treats a third village, Kreuth in Bavaria, the fertility data of which begin first with the 25–29 group (and trend downward over time).

FRANCE. Data in the following local studies point to a fertility increase among young married women during the eighteenth century:

Bayeux (Calvados). Mohamed El Kordi, *Bayeux aux XVII^{e} et XVIII^{e} siècles: Contribution à l'histoire urbaine de la France* (Paris, 1970), 101–03. For women 20–24 and 25–29 who married between 1640–1700 and 1700–80.

Four villages in the Bessin (Calvados). El Kordi, *ibid.,* 139–41. In two of the four villages marital fertility among women 20–24 rose between the periods 1731–80 and 1780–1815.

Bilhères (Basses Pyrénées). Michel Fresel-Lozey, *Histoire démographique d'un village en Béarn: Bilhères-d'Ossau, XVIII^{e}–XIX^{e} siècles* (Bordeaux, 1969), 103. Fertility of women in age groups 15–19, 20–24, 25–29 drops from 1740–79 to 1780–1819, but rises thereafter.

Bléré (Indre-et-Loire). Marcel Lachiver, "Une Etude et quelques esquisses," in Société de démographie historique, *Annales de démographie historique* (Paris, 1969), 215–40. Fertility of 15–19 and 20–24 year-old women who married in 1740–65 was higher than for those married in 1707–39. Fertilities of older women dropped.

Boulay. Jacques Houdaille, "La Population de Boulay (Moselle) avant 1850,"

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Population, 22 (1967): 1055–84, especially 1070–71. Fertility up from 1750–79 to 1810–19 for 20–24 age group. Stable or declining for other age groups.

Seven villages around Boulay. Jacques Houdaille, "La Population de sept villages des environs de Boulay (Moselle) aux XVIII[e] et XIX[e] siècles," *Population,* 26 (1971): 1061–72, especially 1067–68. Same pattern as in Boulay.

Lonrai (Orne). Pierre-Marie Bourdin, "La Plaine d'Alençon et ses bordures forestières: Essai d'histoire démographique et médicale (XVII[e]–XVIII[e] siècles)," *Cahier des Annales de Normandie,* no. 6 (1968): 205–522, especially 262. Fertility higher for those 20–24 year-olds married before 1750 than after. Drops over time for all older women.

Three villages to the east of Paris (Seine). Pierre Rivier and Serge Allegret, "Trois paroisses de l'est parisien: Fontenay, Nogent, Vincennes, 1740–1819" [Summary of *mémoire de maîtrise*], *Dh: bulletin d'information,* no. 4 (1971): 8–19. Fertility of 15–19 year-olds increases for all periods; that of 20–24 group rises until 1789; that of all older groups of women falls.

Sainghin-en-Mélantois (Nord). Raymond Deniel, "La Population d'un village du Nord de la France: Sainghin-en-Mélantois, de 1665 à 1851," *Population,* 20 (1965): 563–602, especially 581. Between 1770–89 and 1820–29 fertility increases for 20–24 year-olds; for age group 25–29 rise 1790–99 to 1820–29.

Saint-Méen. Yves Blayo, "Trois paroisses d'Ille-et-Vilaine," in Société de démographie historique, *Annales de démographie historique* (Paris, 1969), 191–213, especially 200. Big fertility increases for all age groups of women between 1720–55 and 1756–72, except for ages 15–19 and 45–49.

Tourouvre (Orne). Hubert Charbonneau, *Tourouvre-au-Perche aux XVII[e] et XVIII[e] siècles: Etude de démographie historique* (Paris, 1970), 100. Women married in 1715–65 have higher fertility than those married in 1665–1714 at ages 15–19 and 20–24, but not at any other age.

Tamerville (Manche). Philippe Wiel, "Une grosse paroisse du Cotentin aux XVII[e] et XVIII[e] siècles," in Société de démographie historique, *Annales de démographie historique* (Paris, 1969), 136–89, especially 150–51. Fertility increase between 1640–1710 and 1711–92 for 20–24 and 25–29 year-old married women. Decrease for all older groups.

Troarn (Calvados). Michel Bouvet, "Troarn: Etude de démographie historique (XVII[e]–XVIII[e] siècles)," *Cahier des Annales de Normandie,* no. 6 (1968): 17–202, especially 71. Substantial fertility increases from the period before 1760 to the period after 1760 for married women ages 20–24 and 25–29. Declines over time among older women.

Marital fertility of women in their twenties experienced no eighteenth-century increase in the following villages:

Châtillon-sur-Seine (Côte-d'Or). Antoinette Chamoux and Cécile Dauphin, "La Contraception avant la Révolution française: L'exemple de Châtillon-sur-Seine," *Annales: Économies, Sociétés, Civilisations,* 24 (1969): 662–84, especially 666.

Ile-de-France region (Oise). Jean Ganiage, *Trois villages d'Ile-de-France au XVIII[e] siècle: Etude démographique* (Paris, 1963), 85.

Meulan (Yvelines). Marcel Lachiver, *La Population de Meulan du XVII[e] au XIX[e] siècle (vers 1600–1870)* (Paris, 1969), 148–50.

Time-series data for Ingouville (Seine-Maritime) were ambiguous; while the fertility of women aged 20–24 declined between 1730–49 and 1750–70, that of women aged 25–29, 30–34, and 35–39 rose. See Michel Terrisse, "Un Faubourg du Havre: Ingouville," *Population,* 16 (1961): 285–300, especially 287.

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APPENDIX 2:
Prebridal Pregnancy in Selected Localities

Calculations are based on the number of first children born less than eight months after marriage as a percentage of the total number of legitimate births. Only those time series have been included that touch at some point on the last half of the eighteenth century.

Author	*Commune*	*Period*	*Rate of Prebridal Pregnancy*
England			
Hair[a]	77 English parishes	17th century	16.5%
		1750–1836	43.4
Wrigley[b]	Colyton (Devon)	17th–18th centuries	"about a third"
		1800–37	"one half"
North America			
Smith and Hindus[c]	Hingham, Mass.	1641–60	0.0%
		1661–80	7.5
		1681–1700	8.1
		1701–20	10.1
		1721–40	18.0
		1741–60	28.2
		1761–80	31.0
		1781–1800	31.8
		1801–60	decline
Smith and Hindus[c]	Dedham, Mass.	1662–69	4.8%
		1671–80	5.6
		1760–70	28.8
Smith and Hindus[c]	Watertown, Mass.	before 1660	11.1%
		1661–80	5.7
		1681–1700	6.8
		1701–20	15.8
		1721–40	17.7
		1741–60	19.1
		1761–80	22.3
		1781–1800	22.6
Smith and Hindus[c]	Hollis, N. H.	1741–60	10.2%
		1761–80	19.0
		1781–1800	28.1
Smith and Hindus[c]	Kingston parish, Gloucester and Matthews County, Va.	1749–60	13.9%
		1761–70	19.7
		1771–80	17.5
Demos[d]	Bristol, R. I.	1680–1700	0 %
		1700–20	0
		1720–40	10
		1740–60	49
		1760–80	44

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Author	*Commune*	*Period*	*Rate of Prebridal Pregnancy*
North America (cont'd.)			
Somerville[e]	Salem, Mass.	1651–70	5.9%
		1671–1700	8.2
		1701–30	5.8
		1731–70	12.5
Germany			
Houdaille[f]	Boitin (Mecklenburg)	before 1810	28.5%
		1810–39	32.0
Houdaille[f]	Volkshardinghausen (Hesse)	before 1810	20.8%
		1810–39	34.2
Houdaille[f]	Kreuth (Bavaria)	before 1810	2.7%
		1810–39	28.6
Knodel[g]	Anhausen (Bavaria)	1692–1749	14 %
		1750–99	17
		1800–49	20
Borghese[h]	Degerloch (Württemberg)	1800–09	5.9%
		1810–19	18.5
		1820–29	25.0
		1830–39	23.9
		1840–49	26.0
		1850–59	28.2
		1860–69	38.2
		1870–79	27.1
Roller[i]	Durlach (Baden)	1701–30	6.9%
		1731–70	5.3
		1771–1800	16.1
Meyer[j]	an Oldenburg town	1606–1700	21.0%
		1700–80	23.5
		1780–1800	11.4
		1801–50	18.3
Belgium			
Durlet[k]	Jauchelette l'Abbesse (Brabant)	before 1725	0 %
		1725–50	16
		1750–75	37
		1775–1800	62
Deprez[l]	Adegem (Flanders)	before 1660	13.7%
		1660–99	4.8
		1700–39	5.1
		1740–96	6.4
Deprez[l]	Elversele (Flanders)	1608–49	12.5%
		1650–99	14.8
		1700–49	21.5
		1750–96	23.4

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Author	*Commune*	*Period*	*Rate of Prebridal Pregnancy*
France			
Jouan[m]	Villedieu-les-Poëles (Manche)	1711–90	2.6%
		after 1750	increase
Wiel[n]	Tamerville (Manche)	1624–90	9.0%
		1691–1740	11.3
		1741–90	20.0
Charbonneau[o]	Tourouvre (Orne)	17th century	"less than 3 percent"
		1700–50	"3–4 percent"
		after 1750	"10 percent"
Lachiver[p]	Meulan (Yvelines)	1660–1739	"less than 8 percent"
		1740–89	12 %
		1790–1839	24
El Kordi[q]	Bayeux (Calvados)	17th century	8.3%
		18th century	11.2
Fresel-Lozey[r]	Bilhères (Basses Pyrénées)	1740–79	12.9%
		1780–1819	7.7
		1820–59	2.8
Sheppard[s]	Lourmarin (Vaucluse)	1681–90	23.4%
		1691–1700	17.4
		1701–10	33.3
		1711–20	8.2
		1721–30	10.5
		1731–40	13.0
		1741–50	7.4
		1751–60	8.1
		1761–70	13.3
		1771–80	21.0
		1781–90	19.1
		1791–1800	34.4
		1801–10	21.1
		1811–20	20.3
		1821–30	31.7
Houdaille[t]	Boulay (Moselle)	before 1720	6.8%
		1720–49	11.4
		1750–79	13.2
		1780–1809	15.2
		after 1810	28.5
Houdaille[u]	seven villages around Boulay (Moselle)	before 1760	8.6%
		1760–99	7.4
		1800–29	18.4
		1830–62	18.2
Chamoux and Dauphin[v]	Châtillon-sur-Seine (Côte-d'Or)	1772–89	14.7%
		1790–1805	16.9

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Author	*Commune*	*Period*	*Rate of Prebridal Pregnancy*
France (cont'd.)			
Deniel[w]	Sainghin-en-Mélantois (Nord)	1690–1769	15.2%
		1770–89	36.8
		1790–99	42.9
		1800–09	55.6
		1810–19	41.2
		1820–29	58.1
		1830–39	42.6
		1840–49	54.8
Molinier[x]	Sérignan (Hérault)	1716–50	20 %
		1751–92	40

[a] P. E. H. Hair, "Bridal Pregnancy in Rural England in Earlier Centuries," *Population Studies,* 20 (1966–67): 233–43, and Hair, "Bridal Pregnancy in Earlier Rural England, Further Examined," *ibid.,* 24 (1970): 59–70. The exact percentages have been recalculated by Daniel Smith and Michael Hindus on the basis of information Hair supplied in the second article, page 60. Standard: within 8 and one-half months.

[b] E. A. Wrigley, "Family Limitation in Pre-Industrial England," 88. Standard: not mentioned.

[c] Daniel Scott Smith and Michael S. Hindus, "Premarital Pregnancy in America, 1640–1964: An Overview and Interpretation," paper presented at the annual meeting of the American Historical Association, New York, December 29, 1971, pp. 55–57. Standard: within 8 months.

[d] John Demos, "Families in Colonial Bristol, Rhode Island: An Exercise in Historical Demography," *William and Mary Quarterly,* 3d ser., 25 (1968): 40–57, especially 56. Standard: within 8 months.

[e] Cited in Smith and Hindus, "Premarital Pregnancy in America," on basis of unpublished study by Professor James K. Somerville. Standard: within 9 months.

[f] Jacques Houdaille, "Quelques résultats sur la démographie de trois villages d'Allemagne." Standard: less than 8 months.

[g] John Knodel, "Two and a Half Centuries of Demographic History in a Bavarian Village." Standard: within 8 and one-half months.

[h] Arthur Borghese, "Population Patterns in Degerloch, Württemberg, in the Eighteenth and Nineteenth Centuries." Standard: less than 8 months.

[i] Otto Konrad Roller, *Die Einwohnerschaft der Stadt Durlach im 18. Jahrhundert* (Karlsruhe, 1907), cited in Smith and Hindus, "Premarital Pregnancy in America," 62. Standard: less than 7 months.

[j] Erich Meyer, "Beiträge zum Sexualleben der Landjugend," *Zeitschrift für Sexualwissenschaft,* 16 (1929–30): 106–11, especially 108. Standard: not mentioned.

[k] M. Durlet, "La Mortalité à Jauchelette l'Abbesse," *Tablettes du Brabant,* 3 (1958): 82–96, cited in Jean Delumeau, "Démographie d'un port francais . . . ," *XVII[e] siècle,* nos. 86–87 (1970): 3–20, especially 8. Standard: not mentioned.

[l] P. Deprez, "The Demographic Development of Flanders in the Eighteenth Century," in D. V. Glass and D. E. C. Eversley, eds., *Population in History: Essays in Historical Demography* (London, 1965), 608–30, especially 617. Standard: less than 8 months.

[m] Marie-Hélène Jouan, "Les Originalités démographiques d'un bourg artisanal normand au XVIII[e] siècle: Villedieu-les-Poëles (1711–90)," in Société de démographie historique, *Annales de démographie historique* (Paris, 1969), 87–124, especially 107–08. Standard: less than 8 months.

[n] Philippe Wiel, "Une grosse paroisse du Cotentin," 161. Standard: less than 8 months.

[o] Hubert Charbonneau, *Tourouvre-au-Perche,* 142. Standard: less than 8 months.

[p] Marcel Lachiver, *La Population de Meulan,* 173–75. Standard: less than 8 months.

[q] Mohamed El Kordi, *Bayeux,* 91. Standard: less than 8 months.

[r] Michel Fresel-Lozey, *Histoire démographique d'un village en Béarn: Bilhères-d'Ossau,* 143. Standard: less than 8 months.

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[s] Thomas F. Sheppard, *Lourmarin in the Eighteenth Century: A Study of a French Village* (Baltimore, 1971), 41–42. Standard: less than 8 months.

[t] Houdaille, "La Population de Boulay," 1075. Standard: less than 8 months.

[u] Houdaille, "La Population de sept villages des environs de Boulay," 1063. Standard: less than 8 months.

[v] Antoinette Chamoux and Cécile Dauphin, "La Contraception avant la Révolution francaise: L'exemple de Châtillon-sur-Seine," 680. Standard: less than 8 months.

[w] Raymond Deniel, "La Population d'un village du Nord de la France: Sainghin-en-Mélantois," 581. Standard: less than 8 months.

[x] Alain Molinier, "Une Paroisse du Bas-Languedoc, Sérignan, 1650–1792," *Mémoires de la société archéologique de Montpellier,* 12 (1968), cited in Société de démographie historique, *Annales de démographie historique* (Paris, 1970), 421. Standard: not mentioned.

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What about the Dardanelles?

A Review Article by RAYMOND CALLAHAN

MARTIN GILBERT. *Winston S. Churchill.* Volume 3, *The Challenge of War, 1914–1916.* Boston: Houghton Mifflin Company. 1971. Pp. xxxvi, 988. $15.00.

GEORGE H. CASSAR. *The French and the Dardanelles: A Study of Failure in the Conduct of War.* London: George Allen and Unwin. 1971. Pp. xvii, 276. £4.00.

ON MAY 9, 1916, an Irish Nationalist M.P., angered by Winston Churchill's advocacy of extending conscription to Ireland, shouted at him in the House of Commons, "What about the Dardanelles?" thus providing a neat title for the historical controversy that still rumbles on. The nearly simultaneous appearance of two studies, one based on the hitherto unavailable Churchill Papers, provides a good opportunity to consider how close we are to answering the question the Irish nationalist posed.

It now seems clear that the Dardanelles ought not to be considered in isolation from prewar discussions in London about amphibious operations —"warfare upon the littoral," in the jargon of the day. Martin Gilbert examines this topic only very briefly, but it has been fully treated in studies by Arthur Marder and Samuel Williamson.[1] What emerges from their work is that there was little clear thinking in London on amphibious operations and no joint planning for anything except the transport of the British Expeditionary Force to France. Of course the spectacle of service departments working in isolation from—if not actually against—one another is a very familiar one. But there were special reasons for the depths of noncooperation plumbed by the War Office and the Admiralty prior to 1914. Both services were undergoing major reorganizations—particularly the navy, where a veritable revolution was under way in both structure and matériel—and they were planning for very different wars. From 1905 on the army was increasingly committed to a Continental strategy, while the navy, whose strategic concepts were much woolier, still thought

[1] A. J. Marder, *From the Dreadnought to Scapa Flow: The Royal Navy in the Fisher Era* (London, 1961–70), vol. 1; Samuel R. Williamson, *The Politics of Grand Strategy: Britain and France Prepare for War, 1904–1914* (Cambridge, Mass., 1969).

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in "traditional" terms—the destruction of the enemy's fleet in a modern Trafalgar, a blockade to strangle his commerce, and the use of the army in amphibious operations against Germany's Baltic or North Sea coasts: the eighteenth-century formula updated, in fact. The Committee of Imperial Defence (CID) provided a forum where the army and navy could argue the merits of their respective strategies, and it could decide in favor of one over the other, as it did in August 1911 when the Continental strategy of the War Office triumphed over the Admiralty's vagueness. Unfortunately the CID was not designed in such a way that it could provide the machinery for joint planning. Even after its defeat in 1911 the Admiralty still thought in terms of an amphibious strategy, although such plans as were produced reflected the immature and primitive nature of the naval war staff that was imposed on the reluctant admirals by Churchill in 1912.[2] The War Office showed no interest at all in any of these projects. The situation contained the seeds of a major quarrel over strategic priorities if the confident expectation of a quick decision on the western front proved false, while the Admiralty's weak staff work and the utter lack of joint planning boded ill for any attempt to carry out an amphibious strategy.

As the possibility of a quick decision in the west faded away in the autumn of 1914, the search for strategic alternatives began. Many were canvassed, but the Dardanelles was chosen. The process by which this choice was made has hitherto been the weak point in all the accounts of the campaign, as many public and private archives have been closed to researchers.[3] Most of the restrictions on government records have now disappeared, and Gilbert, as Churchill's official biographer, has been able to draw upon a wide range of private papers (he mentions only two collections that were closed to him). The result is a major contribution to our understanding of how the critical decisions were reached. George H. Cassar has also drawn upon government and private records to examine an aspect of the campaign hitherto neglected—the attitude and contribution of the French. Unfortunately there is one great weakness in Gilbert's approach, inherent

[2] The weakness of its staff work was to plague the Admiralty throughout the war. See Marder, *Dreadnought to Scapa Flow, passim,* but especially 5: 313–16. It is interesting to note that Churchill's own attitude toward a Continental versus an amphibious strategy fluctuated considerably prior to 1914. He was an opponent of the army's Continental strategy until the Agadir crisis, when he produced a powerful memorandum arguing in its favor. "Military Aspects of the Continental Problem," Aug. 23, 1911, in Winston S. Churchill, *The World Crisis,* 1 (New York, 1923): 58–63. He was sent to the Admiralty largely to ensure that the sea lords played their allotted—and subordinate—role in the army's plans. By 1913, however, he was calling for plans for amphibious attacks, in the event of hostilities, on a variety of objectives on the coasts of Germany, Norway, Denmark, Sweden, and Holland. Gilbert, *Churchill,* 3: 19–21. Marder, *Dreadnought to Scapa Flow,* 2: 230, provides an interesting commentary on these projects. The documents that complement Gilbert's volume (Martin Gilbert, *Winston S. Churchill: Companion Volume III* [London, 1973]) unfortunately did not appear in time to be used in the preparation of this article.

[3] The most widely read account of the campaign, Alan Moorehead's *Gallipoli* (New York, 1956), is very thin on the origins of the campaign. Even Robert Rhodes James's *Gallipoli* (New York, 1965), though widely acclaimed, is much stronger on the campaign itself than on its origins and completely ignores the role of the French.

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perhaps in his position as a biographer. While he has "consulted all the principal published works covering this period," he never confronts many of the specific criticisms of Admiralty policy and Churchill's role made by other authors, in particular by Marder in the second volume of his monumental *From the Dreadnought to Scapa Flow*. Gilbert's account has to be taken together with Marder's and Cassar's to get a clear picture of the genesis of the most controversial operation of the war. It is true that Gilbert's brief was to write an installment of Churchill's biography, not a definitive history of the campaign, but the Dardanelles is so critical to both the substance and legend of Churchill's career that it is much to be regretted that he did not see fit to deal with it in a different fashion. The long-established tradition by which so much English history is written as biography contains, as in this case, a built-in weakness: it is hard for a biographer to do complete justice to the story as a whole.

The search for an alternative to frontal assaults in the west—"sending our armies to chew barbed wire in Flanders," in Churchill's pungent phrase —began simultaneously on both sides of the Channel, but the initiative in seeking a way around the impasse on the western front had to come from London. In addition to the mobility conferred by sea power, the British had untapped reserves in Kitchener's New Armies—and there were no German troops on British soil. In London, however, the desire to find an alternative strategy went hand-in-hand with considerable disagreement over what it should be. While this disagreement had its roots in the prewar differences between the War Office and the Admiralty, it was now compounded by the strain of war, the personalities of Kitchener and Churchill, and Asquith's unwillingness to impose a clear set of priorities. The Admiralty's attention was concentrated initially on those areas that had been the focus of its prewar planning. Churchill was keen on the seizure of various islands off the North German and Dutch coasts for use as advanced flotilla bases. Operations in the Baltic were conceived on a much larger scale. Churchill had picked up Fisher's prewar ideas on this subject and as early as August 19, 1914, had proposed to the Russian commander in chief, the Grand Duke Nicholas, an attack by a Russian army, convoyed and supported by the Royal Navy, on the German coast ninety miles north of Berlin—one of Fisher's favorite plans since at least 1905. Fisher's return to the Admiralty at the end of October 1914 naturally gave a great impetus to schemes for Baltic operations. Over six hundred new vessels were ordered, many specifically designed for work in the Baltic. Holland was another possible sphere of offensive operations that intrigued Fisher and Churchill. Early in 1915 they were talking about placing an army of 750,000 men there to turn the entire German position in the west.

Gilbert discusses all these projects, but it is to Marder's work that the reader must turn for an analysis of their common weakness: they all disregarded crippling technical problems, problems that a more effective naval

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staff might have successfully pointed out.[4] Moreover, although Kitchener at one point told Churchill that he could find the one division needed for a landing on Borkum, there was no indication that the War Office could, or would, supply the necessary troops for any major amphibious operation. This had been the position of the War Office since 1905, and the pressures of war had not changed the attitude of the general staff or made cooperation between the War Office and the Admiralty any better—rather the reverse. All these defects in planning and coordination were to reappear again in the Dardanelles venture.

The eastern Mediterranean had been considered a possible theater of operations as early as the end of August 1914, when it seemed that a Greek army would be available to seize the Gallipoli Peninsula, opening the way for an attack by the fleet on Constantinople. The head of the British naval mission to Turkey, Rear-Admiral Arthur Limpus, and the military attaché at Constantinople, Major Frederick Cunliffe-Owen, were both reasonably optimistic about the prospects of an attack on the Dardanelles. Two conferences were held between the War Office and the Admiralty to discuss the operation. At the first, the director of military operations, Major-General Charles Callwell, pronounced against the operation; at the second, with Churchill present, he pronounced—cautiously—for it. "How far Churchill prevailed on Callwell to reverse his opinion by the weight of evidence, how far by an assertion of his authority, is not clear," is Gilbert's comment.[5] The project collapsed when the promised Greek army vanished into the quagmire of Greek politics. Interest in the eastern Mediterranean revived again toward the end of the year. Concern was growing over the fate of Serbia, and the reports from the British military mission in Russia were grim. Lloyd George suggested an expedition to Salonika to aid Serbia, coupled with the landing of 100,000 men in Syria. At the meeting of the War Council on November 25, 1914, Churchill and Fisher proposed an attack on the Gallipoli Peninsula. Kitchener toyed with an attack on Alexandretta. The common element in all these projects was a desire to aid Serbia and Russia by striking at Germany's weakest ally. The decisive events, however, were Maurice Hankey's memorandum of December 28 arguing for a concentration against Turkey[6] and the arrival, on January

[4] Marder, *Dreadnought to Scapa Flow*, 2: 176–98. Marder points out that Churchill's memorandums on his project for taking the island of Borkum contain the first mention of the idea that battleships could silence shore batteries. Captain Herbert Richmond of the naval staff explained the weaknesses in this idea, as well as the related problem of clearing mine fields before the shore batteries had been silenced. This was the crux of the problem at the Dardanelles. Unfortunately no one seems to have remembered this exchange of memorandums when the Dardanelles operations were under consideration. *Ibid.*, 188–89.

[5] Gilbert, *Churchill*, 3: 203.

[6] S. W. Roskill discusses this memorandum in his official biography of Hankey, *Hankey: Man of Secrets*, 1 (London, 1970): 149–50. The relevant paragraphs on operations against Turkey are in Gilbert, *Churchill*, 3: 230. Hankey, a Fisher protégé, was a long-time foe of the army's Continental strategy. Marder, *Dreadnought to Scapa Flow*, 1: 394. Gilbert's evidence demonstrates the full extent of Hankey's role in the formative stages of the Dardanelles operation. In par-

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2, 1915, of an appeal from the grand duke for a diversion by his allies to take the pressure off the Russian armies in the Caucasus. Kitchener had no troops available, but he suggested to Churchill that a naval demonstration against the Dardanelles would be of value.

At this point it is again necessary to supplement Gilbert's account with Marder's.[7] Gilbert places great emphasis (as did Churchill himself before the Dardanelles Commission and later in the second volume of *The World Crisis*) on the response by Vice-Admiral Sackville Carden, commanding the squadron off the Dardanelles, to Churchill's telegram of January 3, 1915, enquiring whether he thought the Dardanelles could be forced. But, as Marder points out, Carden was distinctly "second eleven," a view fully shared by Churchill at the time. Carden was in command solely because Grey, before the outbreak of hostilities with Turkey, had vetoed the appointment of Limpus as unduly provocative to the Turks—a decision Churchill strongly protested. The "plan" Carden submitted on January 11 was in fact no such thing, but merely a vague statement of intention. Gilbert does not deal with the charge, first made before the Dardanelles Commission and documented in great detail by Marder, that Churchill ignored dissenting opinions or failed to consult those who might have disagreed. Gilbert makes the point that when Carden's plan was first discussed none of those who subsequently criticized it voiced any doubts. But this ignores Admiral Sir Henry Jackson's later claim that Churchill, in citing him as a supporter of Carden's plan, "probably stretched to the extreme" what the admiral had actually said.[8] Gilbert's observations on Churchill's meeting with General Callwell may apply with even greater force to the discussions within the Admiralty in January 1915. Despite the wealth of documentation he has produced, Gilbert does not completely succeed in dispelling the feeling that, as Alan Moorehead put it, Churchill "bamboozled the Admirals."[9] Churchill's well-known enthusiasm for the task in hand seems to have taken control in January 1915.

On another controverted point—the attempt to force the Dardanelles with ships alone—Gilbert's narrative carries much more conviction. Churchill believed in the necessity of an amphibious operation to secure the Gallipoli Peninsula when the subject was first raised in August, but he had swung over to the idea of a purely naval attack in January. By February

ticular he cites Hankey's intervention (which Roskill does not mention) at the meeting of the War Council on January 8, 1915, to support Kitchener's plea for operations at the Dardanelles, and Hankey's long memorandum of March 2, 1915, for the War Council on the way in which victory at the Dardanelles ought to be followed up. *Churchill*, 3: 244, 318–20. The latter memorandum is mentioned but not reproduced by Roskill. Hankey's memorandum of September 1, 1916, to the Dardanelles Commission, explaining why the adverse opinion on an attack on Gallipoli recorded by the CID in 1907 was set aside by the War Council in February 1915, is reproduced by Gilbert on pages 294–95, but finds no place in Roskill's volume.

[7] Marder, *Dreadnought to Scapa Flow*, 2: 199–228, 259–65.

[8] Jackson's evidence before the Dardanelles Commission is quoted in *ibid.*, 223.

[9] Moorehead, *Gallipoli*, 46.

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he was having second thoughts. So was nearly everyone in the Admiralty. Their doubts were communicated to Hankey and through him reached Asquith. But no troops were available. Although Kitchener was one of the principal advocates of the Dardanelles operation, he would not release his last regular division, the Twenty-ninth, and everyone agreed that the raw ANZAC forces in Egypt needed the "stiffening" that regulars would provide (in the event, of course, they did very well without it). Asquith would not overrule "K of K." Cassar points out that Joffre was pressing Kitchener to keep the Twenty-ninth Division in the west. Did Joffre really believe one division, even of regulars, would make a critical difference? Or was he making the Twenty-ninth the symbol of his allies' commitment to the western front—and his own continued dominance over the strategy of the Entente? Joffre, as Cassar points out, was a consummate strategist when his own position was threatened. Furthermore, Gilbert records a major row, at the crucial moment in February, between Churchill and Kitchener over what the secretary of state for war regarded as an intrusion by the first lord (who had a bad record in this respect) into the sphere of the War Office. Whatever the respective weight Kitchener gave to his own personal pique, Joffre's representations, or the War Office bias toward the western front, the Twenty-ninth Division, which he had offered on February 16 for use at the Dardanelles, was taken back three days later. Defeated in his attempt to pry it from Kitchener's hands, Churchill warned the War Council on February 26: "If a disaster occurred in Turkey owing to an insufficiency of troops, he [Churchill] must disclaim all responsibility." He carried on, nevertheless, with plans for a purely naval assault. It is hard to quarrel with Gilbert's conclusion that Churchill

> now believed that ships alone would be inadequate, . . . But he so believed in the need for victory that he was prepared to go ahead with the plans for a purely naval attack. However much he continued to argue that these plans might fail, by agreeing to go ahead with them, he made himself responsible for the very disaster he forecast.[10]

The atmosphere of confusion in which the critical decisions were made was a faithful reflection of the weakness of Asquith's machinery for direction of the war. Neither the cabinet nor the War Council was any better able than had been the prewar CID to ensure orderly consideration of alternative strategies or proper joint planning. Asquith, although "the final arbiter of war policy," was not by temperament a war leader and did not have at his disposal the machinery to focus properly the problems that came before him for consideration. In fact at the center there was a vacuum. Critical decisions were taken in an atmosphere that was casual to the point of inanity. Ministers talked, or argued, at large, while

[10] War Council minutes, Feb. 26, 1915, quoted in Gilbert, *Churchill*, 3: 310; quotation from page 311.

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Asquith presided. "When he thought there had been talk enough," noted Vice-Admiral Sir Henry Oliver, the chief of the naval staff, "he would look up and say 'So-and-So is decided.'"[11] The accuracy of this description is confirmed by one of Asquith's letters to Venetia Stanley: "I maintained an almost unbroken silence until the end, when I intervened with my conclusions."[12] This letter was written after the meeting of the War Council on January 13, 1915, that decided to initiate a naval operation against the Dardanelles. Gilbert's extensive quotations from Asquith's letters to Venetia Stanley are a rather telling blow at an already somewhat eroded reputation. So much of the discussion about the responsibility for the Dardanelles has centered on Churchill—he was so visible at the time and so obsessively insistent on justifying it all afterward—that the figures who sat with him on the War Council have tended to fall into an entirely unwarranted shadow. One of the great merits of Gilbert's book is the proper perspective the author puts on the roles of Asquith, Kitchener, Grey, and Fisher, who justifiably emerges more battered from Gilbert's handling than he does from Marder's. It was, however, above all the deficiencies in the machinery for conducting war that allowed personal and accidental factors to bulk so large in the Dardanelles story. Churchill wrote many years later, "I was ruined for the time being in 1915 over the Dardanelles and a supreme enterprise cast away, through my trying to carry out a major and cardinal operation of war from a subordinate position. Men are ill advised to try such ventures."[13] He was only partly correct. It was his misfortune, and that of many others, that in 1915 an operation was attempted which required the sort of machinery for interservice and interallied cooperation available in 1940–45.

If clarity in conception and coordination in planning were not very evident in London, cooperation with the French was in an even worse state. By the late autumn of 1914 alternative strategies were being actively discussed in France. Dissatisfaction with the primacy of the western front and the dominance it gave Joffre and his staff over strategy was becoming widespread in French political circles. The unremitting hostility of Joffre to "side shows" owed a great deal to his realization that they were intended to outflank him as well as the enemy. The alternative originally suggested in Paris was an expedition to Salonika to sustain Serbia, rally the Balkans to the Allies, and threaten Austria-Hungary from the south. The French were drawn into the Dardanelles for different reasons. The eastern Mediterranean, by an Anglo-French agreement of August 6, 1914, was a sphere of French naval command. The forcing of the Dardanelles would be a predominantly English venture, and command arrangements had to be revised accordingly. France had considerable economic interests in Turkey and Syria and

[11] Quoted in Marder, *Dreadnought to Scapa Flow*, 2: 222.
[12] Quoted in Gilbert, *Churchill*, 3: 253.
[13] Churchill, *The Second World War*, 2 (Boston, 1949): 15.

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future plans for the latter. Any British move in the eastern Mediterranean was therefore viewed in Paris with considerable suspicion. Victor Augagneur, the French minister of marine, went to London on January 26, 1915, and drove a bargain with Churchill. Cassar states that no written record of the discussion was preserved, but Gilbert prints a memorandum from the Churchill Papers, outlining the French terms. The French would accept the Dardanelles and Egyptian waters (defined, interestingly, as extending as far north as Jaffa) as areas of British command. The Levant from Jaffa to Alexandretta was to be under a French vice-admiral.[14] Although concerned with naval command, we can see here the outline of the following year's Sykes-Picot agreement and the postwar division of the Middle East. The French joined the expedition primarily to keep an eye on the British, because, as Augagneur subsequently told the marine commission of the assembly, "to witness the appearance of the English fleet alone before Constantinople would have been a very painful renunciation of our national pride and perilous for our interests."[15] This being the case, expert service opinion was beside the point, and the French admirals were treated more brusquely by Augagneur than Churchill ever dared treat his sea lords. The chief of the French naval staff was not consulted until after Augagneur had committed the French navy. The French commander in chief in the Mediterranean had to ask Paris for the details of the operation after he discovered from one of his subordinates that it was in train. There was no combined planning. There was a French presence, dictated by political necessity rather than a French commitment to the success of the campaign. Once an Anglo-French force was established at Salonika, where it performed an important function in French politics, if not its ostensible purpose of aiding Serbia, the French threw their weight against prolonging the Gallipoli campaign. They may have played a critical role in forcing the final evacuation of the peninsula. Cassar lays before us a classic example of the not always happy interaction between politics and strategy.

The Dardanelles campaign was the only major attempt to move the focus of Allied strategy away from the western front. Its failure left the "westerners" firmly in control for the remainder of the war. It is the three years after 1915 that invest the Dardanelles operation with its fascination. Might it have shortened the war and, perhaps, changed the course of history? This question will keep historians coming back to those months in 1914–15, to our understanding of which both Gilbert and Cassar have made major contributions.

[14] Cassar, *The French and the Dardanelles*, 57–59; Gilbert, *Churchill*, 3: 266–67.
[15] Quoted in Cassar, *The French and the Dardanelles*, 60.

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Reviews of Books

GENERAL

RICARDO J. QUINONES. *The Renaissance Discovery of Time.* (Harvard Studies in Comparative Literature, 31.) Cambridge, Mass.: Harvard University Press. 1972. Pp. xvi, 549. $15.00.

Ricardo Quinones has managed to convince at least one skeptical reader that the real voyages of discovery during the Renaissance took place in the European mind rather than off the coasts of tropical America. Time itself provides the theme and the organizing structure for this study of Dante, Petrarch, Rabelais, Montaigne, Spenser, Shakespeare, and Milton. And although the rubric of "comparative literature" is accurate, and Quinones a sensitive literary critic, the book is essentially a contribution to the history of ideas.

In the new world of the mind that the Renaissance uncovered it almost seems as though fame displaced salvation, children became more important than bliss eternal, the classical virtues outstripped saintly virtues, and education replaced religious discipline (note the curious correspondence between the sacraments and various stages of academic exaltation). The informing principles underlying these changes were two: simply, God's order had been replaced by history (man's order) and God's Grace by Fortuna. The studies of Dante and Petrarch prepare the way for part 2 of the book, which treats of history and tragedy in sixteenth-century thought. And it is here that we move fully into man's domain, for if God's limits had not always been well defined, there were optimistic certainties, chief among them the triumph of Christian virtue. *Virtù*, however, was to be another matter in the earthly city where the potential for both good and evil would be greatly expanded. The author indicates that Machiavelli saw the double-edged nature of the temporal sword that man alone would wield. If possibility seemed to beckon with the freshness of a new rose, so, too, skepticism would finally impale with one thorn that could have been hewed from the True Cross itself. Man would realize the final tragedy of his despair before his own pitiful limits.

And while the complexity and richness of Quinones's insights can only become evident when one reads his book, it might be argued that he has shown the origins of the dilemma that some existential writers call freedom (or choice). Perhaps the author is saying that when man takes time into his own hands, when man seeks totally to control his day, then he will be obliged to keep vigil in his own night and awake to his own dawn.

ALICIA BETSY EDWARDS
George Washington University

ARTHUR M. WILSON. *Diderot.* New York: Oxford University Press. 1972. Pp. xviii, 917. $25.00.

The first part of Professor Wilson's monumental biography of Denis Diderot (published under the title *Diderot: The Testing Years, 1713–1759* [1957]) ended with an epilogue entitled "The Nature of the Ultimate Triumph" and thus opened a perspective toward the future, a perspective that is also indicated in the title of part 2. The "ultimate triumph" was not only the publication of the *Encyclopédie* but also Diderot's emergence from an inner crisis caused by fierce attacks on his works and his person. As Professor Wilson stresses, the test brought about also a maturation of Diderot's

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intellectual gifts and a strengthening of his moral fiber. Still, the relationship between the author and his public had been deeply affected. Diderot published little after the termination of the *Encyclopédie*; most of the works that we now consider his masterpieces were known only in the eighteenth century to a small circle of readers and were published posthumously. The Encyclopedist, one might say, had acquitted himself of his duties toward his contemporaries; the philosopher and man of letters wrote for the approval of posterity, an approval that he compared to the charm of sounds from a distant concert of flutes, performed at night. Posterity, indeed, did not forget Diderot, and its testimony, as the present book shows by itself (710 concentrated pages of biography and intellectual history) and by its scholarly apparatus (170 pages of notes in fine print), has rather the proportions of a full orchestra and chorus.

Professor Wilson's thorough account of Diderot's life and the world in which he lived is exemplary; his detailed presentation of Diderot's ideas is all the more informative and instructive as it is set into the broad framework of the eighteenth-century movement of ideas. The dichotomy of "the man and the work" is carefully avoided; biography and intellectual as well as artistic history are interwoven. This is no small achievement, as everyone who has tried his hand at it well knows. From a strictly philosophical point of view, one might say that the biographical element occasionally prevails and that the intellectual issues are at times oversimplified and treated summarily. Literary issues also suffer in some instances from a biographical-anecdotal bias. To give an example: Professor Wilson takes a rather personal stand on Diderot's remark, "il n'y a que le méchant qui soit seul," a remark that Rousseau applied to himself. The author exclaims: "One has the right to ask Diderot . . . what precisely did he mean by so publicly and so gratuitously remarking that only the evil man lives alone" (p. 259). The literary historian would notice first of all that the line is taken out of context, with regard to the passage in which it occurs, to the scene and to the entire play (*Le Fils naturel*), which serves to illustrate the poetics and ethics of the bourgeois drama, based on a social philosophy. The main tenet of this genre and of its philosophy is virtue, which implies a sacrifice of oneself and the triumph over an asocial disposition. Inasmuch as Rousseau was a radical critic of society, the remark applied also to him. The "méchant" in the line from *Le Fils naturel* is the misanthrope and a typified Timon of Athens, who frequently are mentioned in discussions of the new social ethics in the eighteenth century.

Some readers might also be surprised by an occasional flippancy in Professor Wilson's comments. Naigeon, Diderot's friend and disciple, to whom we owe the first outstanding edition of Diderot's works and the first comprehensive evaluation of Diderot's thought, is introduced as "a bore, long-winded, tiresome, and steeped in a particularly dull and monotonous form of atheism" (p. 496). Of Diderot's relationship to Mme de Meaux ("a shadowy figure," p. 572), Professor Wilson says that it caused Diderot "to write some of his best stories and dialogues, all of them focused on problems of sexuality and the sociology of sex and the inscrutability of love." The only story in which Mme de Meaux had a part is *Les deux amis de Bourbonne*, in which sexuality and the sociology of sex are notably absent.

Professor Wilson's idea of listing the biographical data in the notes to the text is fortunate indeed. If he had listed separately all the books and articles that have been written on Diderot, the reader would have been bewildered by the multitude of listings (by far the most extensive in existence), and the student or scholar looking for a topic inviting further research would have felt crushed. As it is presented, the bibliographical information is integrated in the biographical and intellectual history and is enlivened by it.

One of the main concerns of the author is to introduce Diderot to a broad public without sacrificing the thoroughness and accuracy that the subject matter requires. The task is difficult, and Professor Wilson is to be commended highly for his achievement. Some readers may find his style too popular at times and somewhat chatty, his humor too jovial, but this is a matter of personal taste, a question on which the learned disagree widely and constantly. In the meantime Professor Wilson can say with

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the *Direktor* in Goethe's *Faust:* "Wer vieles bringt, wird manchem etwas bringen" (Vorspiel).

HERBERT DIECKMANN
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J. D. BERNAL. *The Extension of Man: A History of Physics before the Quantum.* Cambridge, Mass.: M.I.T. Press, Massachusetts Institute of Technology. 1972. Pp. 317. $12.50.

The late J. D. Bernal was one of Britain's foremost scientists whose researches in X-ray crystallography contributed significantly to the discovery of the structure of the DNA molecule. He was also a prolific historian of science whose *Science in History* (1954) presented the entire panorama of the development of the sciences from a Marxist perspective. *The Extension of Man* is Professor Bernal's last book, and it represents his reworking of the lectures he delivered to first-year students of physics at Birkbeck College from 1946 until his death. It is difficult to describe this work accurately. Neither the subtitle on the dust jacket, *The History of Physics before the Modern Age,* nor the subtitle on the title page, *A History of Physics before the Quantum,* is quite right. Bernal does not deal adequately with theoretical physics, nor does he bring his story down to the origin of quantum theory. Much of the text is devoted to technology, and I think that Bernal would not be unhappy if this work were described as a social history of invention, with emphasis upon experimental physics. What he actually deals with is man's confrontation with his environment and his technological response to it. When this response has given rise to ideas that we recognize as scientific, Bernal gives an account of these ideas; when no such ideas are forthcoming, he presents the technological achievement and follows this with a Marxist analysis of the failure to conceptualize it and discover general laws.

It is difficult to understand for whom this work is intended. It will be old hat for anyone who has read *Science in History.* By now many of the positions and arguments that were novel and challenging in the earlier work have either been generally accepted or refuted. Bernal's discussion of medieval technology, for example, lags far behind what Lynn White has been saying for years. Similarly, the treatment of the nineteenth century is not only brief but quite inferior to what Bernal said in *Science in History* and greatly out of date. Thus, the book will not be read seriously by scholars. I cannot believe that it will attract many students. Parts of it read like *1066 and All That.* One short example must suffice. Under pneumatics in antiquity Bernal writes that "when the Greek fleet was ready to start for Troy, during the month of August, there was no wind—and still no wind: something really had to be done. So Commander-in-Chief Agamemnon's daughter, Iphigenia, was sacrificed in order, as he thought, to ensure a good wind. He got his wind but subsequently he was murdered." All that is missing is the tag line that "this was a good thing." Such style may be effective when lecturing (although the syntax is bothersome), but it reads poorly.

Bernal's story is also marred by errors of fact that cry out for correction. Ancient historians will wince at the statement that "in the fifth century B.C. the Persians conquered Greece" (p. 71); the Pythagorean theorem was not known "many thousands of years before Pythagoras" (p. 76); the Museum of Alexandria was not founded by Theophrastus (p. 92), and so on.

Serious students of the history of science and admirers of J. D. Bernal would be well advised to ignore this work and order the newest edition of *Science in History* or *The Social Function of Science,* which are also published by the MIT Press.

L. P. WILLIAMS
Cornell University

GEORGE F. BASS *et al. A History of Seafaring: Based on Underwater Archaeology.* Edited by GEORGE F. BASS. New York: Walker and Company. 1972. Pp. 320. $22.50.

Historians interested in maritime development in recent years have become aware of a body of new information that archeology in general and underwater archeology in particular have been making available to scholars. It is particularly important for them, therefore, to pay attention to this handsome book, which manages to sum up, through the accounts of experts, some results of this kind of discovery and some

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of the tentative conclusions that can be drawn from them.

The book itself consists of a series of chapters dealing with the earliest period in the Near East, with Greek, Etruscan, and Phoenician ships and shipping, with the Roman period in the Mediterranean, with Britain through the Roman and early migration period, with Byzantium down to 641, with Scandinavia and Northern Europe down to 1250, with medieval and Renaissance Italy, with British shipping from 1400 to 1850, with the Atlantic between 1492 and 1733, and with the New World of North America and especially the area of the Great Lakes. Each chapter attempts to integrate what has recently been learned from archeology with other information available concerning ships and ship design.

Though the results are disappointing in places because underwater and maritime archeology have been concentrated almost exclusively so far in Europe in the North Sea and in the Baltic area and in the central and eastern Mediterranean to the almost total neglect of the Black Sea, the waters off the coasts of the Iberian peninsula, or the Atlantic coasts of France, one can now discern some important information which can be derived from the discoveries that have already been made, some of which seem worth summarizing here.

First of all, the authors of this book make it clear, using evidence from a number of sources and from the wreck of an early seventh-century Byzantine ship at Yassi Ada off Turkey, that during the sixth and seventh centuries a technological revolution in shipbuilding took place in the Mediterranean, a revolution that helps to explain the maritime empire of Justinian and his early Byzantine successors. Second, they reveal a pre-Roman shipbuilding tradition in Britain and on the nearby Continent stressing carvel-built ships that may well have continued in Atlantic waters off the coast of France, where a very different *barca* or barge continued to be used down to the fourteenth century. Third, the authors show us that the hulk and the very different cog originated in Carolingian times in the North Sea along the coasts of Frisia and the Netherlands. In the fourth place they reveal to us exactly what the Scandinavian *knorr* and the somewhat similar Wendish ships of the Baltic were like. And last of all they do much to show us how the cog developed during the twelfth and thirteenth centuries and then changed later on in its construction to become the carrack.

A number of points, however, seem to be in order concerning some of the conclusions these authors have drawn from their data. First of all, one is surprised to find no reference to the different construction in England and Scandinavia of that great warship, the *drekkar,* whose high castle anticipated those used later on cogs. Second, the large ships discovered at Bergen and elsewhere in Scandinavia were not necessarily commercial ships, as is claimed, but more probably warships that we know Norway's king used off the Hebrides during the mid-thirteenth century. It is also a pity that Catalan ships, about which we know a great deal, are not mentioned by the authors, who seem unaware that these mariners used cogs long before the time Villani mentions them in the Mediterranean. And finally one misses some reference to the fact that a type of compass was in use in late twelfth-century England.

Such criticism, however, should not deter us from welcoming this handsome book, so profusely illustrated and containing an excellent critical bibliography. No doubt the picture of maritime development presented by its authors will change as more underwater and other discoveries are made, but we will remain much in debt to Professor Bass and his collaborators for adding to our knowledge of a fascinating and important subject.

ARCHIBALD R. LEWIS
University of Massachusetts, Amherst

EDMUND W. GILBERT. *British Pioneers in Geography.* New York: Barnes and Noble. 1972. Pp. 271. $12.50.

Articles and addresses representing over thirty years of distinguished scholarship constitute this volume by the professor emeritus of geography at Oxford. They demonstrate his long interest in the history of geographical thought and teaching, in particular at his own university, and his distrust of the quantifiers and model builders who have arrived in his discipline and who "often adopt a strangely uncivil-

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ized attitude to the history of geographical ideas."

Gilbert's own view is broadly humane, tinged perhaps by a slight antiquarianism. Geographers and especially teachers of geography are the subject of his twelve chapters, and their variety illustrates the breadth of his perception of the discipline. First, as might be expected, comes Richard Hakluyt and other early geographers at Oxford. They are followed, however, by a group of seventeenth-century Oxford divines who wrote on geography; Richard Kane, an early eighteenth-century British colonial administrator; some early nineteenth-century pioneers in medical geography; Richard Ford, the traveler in Spain; and some British regional novelists whose descriptions came to characterize urban as well as rural England.

The teachers, however, evoke Gilbert's greatest enthusiasm. After a description of geography teaching in Britain in the early nineteenth century—"largely learning by rote the names of places and products" (a method that continued well into the twentieth century in at least one part of Canada)—he deals with the heroes of the book, Sir Halford Mackinder, Andrew John Herbertson, and Percy Maude Roxby. Their work at Oxford and elsewhere helped revolutionize the teaching of geography in this century.

A final chapter on Vaughan Cornish, a geographer of independent means who devoted the later part of his life to emphasizing the beauty of scenery, points the way to a modern emphasis on the environment and the dangers facing it in our industrial society.

D. G. G. KERR
University of Western Ontario

JOSEPH AGASSI. *Faraday as a Natural Philosopher.* Chicago: University of Chicago Press. 1971. Pp. xiv, 359. $12.50.

Agassi uses Faraday to illustrate his own philosophy of science, proclaiming that it is better to make an interesting error, which may be read and rectified, "than a dull truth that will stay put like a heavy tombstone in a forgotten graveyard." He succeeds beyond his wildest dreams to do plenty of both and yet manages to produce a book that is both delightful and exasperating.

In part, his difficulties stem from the fact that he wrote most of the book in 1956, while still a graduate student, and when Alexandre Koyre was the biggest star shining in the very small field of the history of science. Using him and Agassi's philosophical mentor, Karl Popper, as his models, Agassi flays Faraday's biographers. But are they truly representative of historians? Several were friends of Faraday and/or scientists. Does that turn them into qualified biographers? Their faults should not be generalized and attributed to all historians of science. Reading this and seeing how Agassi flogs dead horses to a second death is exasperating. Clearly, these men were not in a position to produce the up-to-date philosopher that Agassi sees in Faraday. Equally nerve-racking is Agassi's reading of Faraday into the work of others. Einstein's general theory of relativity, for example, is the most Faradayan theory still extant (p. 227). Faraday anticipated Bohr's theory of the atomic structure of 1913 (p. 219), or Faraday's suggestion about atoms comes "astonishingly near to Schrödinger's theory of the material waves" (p. 223).

Agassi is more successful when he presents two portraits of Faraday, the private and the public, and attempts to integrate the two. Faraday's peers viewed him predominantly as a discoverer of new facts about nature. Agassi argues that Faraday's own perception was different and that he considered that his contributions of a philosophical nature were more important than his discoveries. Faraday's boldest philosophical contribution was to allow for vibrations without a vibrating matter, viewing matter as fields of forces. As a thinker Faraday was ostracized by his peers. Agassi argues that this isolation had social roots and that class discrimination had led Faraday, as early as 1821–23, to severe disappointments as to the seriousness with which his philosophical pronouncements were received. Yet Agassi turns Faraday's experience into a unique example when it really was a widespread phenomenon. It is here that Agassi, by way of opening up new territory, deserves our gratitude and manages to produce a bold new look at Faraday.

ROMUALDAS SVIEDRYS
Polytechnic Institute of Brooklyn

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F. R. HODSON *et al.*, editors. *Mathematics in the Archaeological and Historical Sciences: Proceedings of the Anglo-Romanian Conference, Mamaia 1970, Organized by the Royal Society of London and the Academy of the Socialist Republic of Romania.* Edinburgh: University Press; distrib. by Aldine-Atherton, Chicago. 1971. Pp. vii, 565. $36.00.

At last we have some meaty reading for the mathematician turned historian. While these proceedings of a 1970 Anglo-Romanian conference are oriented primarily to quantitative archeologists and linguists, there is some material of interest here for avant-garde specialists in demography, roll call analysis, and automated record linkage. More traditional and less interesting are ecological correlation studies of nineteenth-century East European agriculture.

The mathematics is of a higher order than the history. The material will be bewildering to anyone unfamiliar with scaling or cluster analysis. The papers on numerical taxonomy by Herbert Solomon and multidimensional scaling by Joseph Kruskal are valuable overviews of two rapidly growing areas of importance to historiography. Five reports by the Cavalli-Sforza team discuss their important work in automated record linkage (an explosively growing field of central importance to social history) with reference to Italian parish registers and their research on reconstructing evolutionary sequences. It is fascinating to see the zoologists' work on deducing the steps of animal evolution from taxonomic trees applied to historical demography. Thus far efforts to date branching sequences in history have not been successful, though reports in this volume by archeologists and linguists indicate they are having better luck.

Nondated historical processes turn out to be more susceptible to mathematical modeling. Robert Hiorns's models of migration among Oxfordshire parishes, using intermarriage rates, are relevant to social stratification even though they were originally designed for population genetics research. The most intriguing paper is by David Kendall, who proves that geographical maps can be drawn by applying nonmetric multidimensional scaling to parish register data. Kendall even provides a method for locating lost villages. A good map of a group of parishes can be drawn if the distance between each pair is known. The distances used do not have to be in miles, since nonmetric scaling will permit the historian to use instead the rate of intermarriage between pairs of parishes. These intermarriage rates can be calculated for lost villages on the basis of surviving registers; the pairs of distances can be estimated; and the approximate location of the lost village can thus be circled on a map. Now if only the mathematicians can devise a method to locate lost letters!

RICHARD J. JENSEN
University of Illinois, Chicago Circle

PETER BROCK. *Pacifism in Europe to 1914.* (A History of Pacifism, Volume 1.) Princeton: Princeton University Press. 1972. Pp. x, 556. $17.50.

PETER BROCK. *Twentieth-Century Pacifism.* (New Perspectives in Political Science.) New York: Van Nostrand Reinhold Company. 1970. Pp. vii, 274. $3.50.

FREDERICK J. LIBBY. *To End War: The Story of the National Council for Prevention of War.* Nyack, N.Y.: Fellowship Publications. 1969. Pp. xiii, 188. $5.00.

Pacifism in Europe to 1914 completes an impressive quartet of works tracing in depth that approach to peace which requires personal repudiation of war—either in obedience to the religious discipleship that defines a particular community or in an impulse to challenge the destructive policies of society. Building upon his 1957 monograph, *The Political and Social Doctrines of the Czech Brethren in the Fifteenth and Early Sixteenth Centuries,* Brock treats pacifists from the early Christians to the later Tolstoyans. His *Pacifism in the United States to 1914* (1968) recounted the history of American nonresistants in rather overwhelming detail; his *Twentieth-Century Pacifism* (1970) almost breathlessly surveyed the modern variants of pacifism. Given the differences in scope and audience of these books and the fact that they were not written in the chronological order of their subject, there is an understandable disparity between them. *Pacifism in Europe* is the best written and probably will have the most enduring significance.

This is a book of mature scholarship, the product of a considered use of reliable secondary sources tested and much supplemented by

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primary research. Its bibliographical notes are a model of selective scholarship. The book is analytical with regard to pacifists' thought but narrative with respect to their organizations. The author freely acknowledges the variety of positions taken by men constrained by both their faith and their circumstances, but he keeps the story firmly under control. He establishes the fact that there are unbridged gaps and recurring geneses of pacifism until the early modern period. He relates the subtle movement of faith in a succession of groups with similar social and religious origins; and he unfolds not only a recurring pattern of germination and decay, separatism and secularism, with regard to the issue of violence, but also a cumulative shift in the rationale and attention of pacifists in the modern period when social involvement increasingly characterized their efforts. In this respect, the typology with which Brock concludes grows out of the historical experiences of pacifists and provides more useful categories of understanding than the sociological constructs of, for example, David Martin (*Pacifism*, 1966).

Brock's categories provide a transition to his *Twentieth-Century Pacifism*. Based on wide reading in secondary and published primary sources this book is unique in carrying the story of pacifism into the nuclear age from a world view. Even so, it is heavily oriented to the Anglo-American experience. The author neglects the pre–World War I pacifist tradition in Holland and the persisting Tolstoyan influence on Czech pacifists, perhaps; and he does not explore the tangled relations of pacifists, peace advocates, and socialists in Germany and Switzerland. Necessarily, he omits pivotal individuals and some organizations in his survey.

The strength of the book is its balanced and analytical framework. Whereas the sectarians had faced pressures to modify their pacifism in the process of socialization, socially conscious pacifists of this century have been frustrated in their search for pressure points at which they might influence social change. The book delineates this theme on an international scale, and it makes useful undergraduate reading. If its treatment of some of the relationships of pacifism to the broader peace and antiwar movements is cursory, then it outlines a program of peace research in modern history.

To End War should have been a model case in point. Written by Frederick J. Libby (1874–1970), pacifist leader of the pre-eminent peace lobby between the world wars, it recounts the hitherto fragmentary story of that group, the National Council for Prevention of War (NCPW). The NCPW had its origins in the disarmament campaign following World War I. It actively challenged rearmament and mobilized public support for arms control and strict neutrality. It was the principal agent of a historic antiwar coalition engaged in political activity. Libby's autobiographical account gives continuity to his organization and insight into its operation. But it is not sufficient. Accounts of the contexts of NCPW work—the Washington Naval Conference, for example—are based on limited secondary sources. Much is omitted of the NCPW's work in the thirties and after World War II (when in 1949, for instance, it played a key role in publicizing the Malmedy murders). Most important, Libby left out too much of himself, of the intensity of his conviction and activism, of the contours of his thought. The book is an important resource, but it is not definitive. The political organization is to the modern pacifist what the religious community was to the sectarian, so that in modern no less than in early times we are led with Peter Brock to the further study of those processes of germination and decay that somehow link otherwise disparate organizations of outrage against war.

CHARLES CHATFIELD
Wittenberg University

MARY BETH NORTON. *The British-Americans: The Loyalist Exiles in England, 1774–1789*. Boston: Little, Brown and Company. 1972. Pp. x, 333. $12.50.

While it is unrealistic to expect the complex subject of American loyalism to be covered comprehensively in a single volume, *The British-Americans* is nevertheless the most satisfactory book on the subject yet to appear. It is well written and rests upon solid source materials painstakingly researched. Although specifically focused on only those loyalists who took refuge in England (around 7,000), it is a thoughtful study that probes numerous facets of their plight and adds important dimensions

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to our understanding of the loyalist experience.

Norton begins with a survey of circumstances that initially drove numerous Americans to England for safety, and in succeeding chapters she analyzes the motivation of individual loyalists, their life and activities in London, their treatment by the government, their changing fortunes and attitudes as the war unfolded, their consternation at the peace negotiations, their reaction to provisions made for their support, and their eventual attempts to find permanent homes. Fortunately the work is also a happy combination of analytical and narrative history, which reflects the author's familiarity with her materials, her mature judgment, and her sane conclusions about the meaning of the loyalists' experiences. For example she warns at the outset that it is "practically impossible to extrapolate backwards from an analysis of the refugees to obtain an accurate picture of the loyalist community as a whole" (p. 39). And throughout she emphasizes the wide range of individual circumstances, avoids excessive generalization, and roots her conclusions in a solid framework of events and chronology.

A brief review can suggest neither the balance nor richness of Norton's work. Her analysis of the writings of the loyalists in London, if fleshed out, would make a book in itself exceeding any other treatment of the subject, while her coverage of the evolution of the British government's pension and claims policies is truly excellent. And occasionally, such as in contrasting the provincialism of the loyalists in London with the rebels' achievement of unification in America, as well as in distinguishing between the various approaches the government successively employed to deal with the tide of refugees, she exhibits flashes of exceptional insight.

Yet the book is inescapably traditional history in the sense that it is a view of the loyalists from the top down, based overwhelmingly upon the writings of the loyalist elite and British official records. The wives and children of even the most prominent loyalists seldom appear on stage, and no real attempt is made to grapple with thousands of inarticulate refugees who quietly submitted to their fate. Finally at least one flaw invites specific mention: the consistent overemphasis of the loyalists' influence on ministerial plans for conducting the war in America. While the loyalists were in fact pawns of a troubled ministry desperately seeking to maintain parliamentary support for the war, they are often portrayed as the dominant influence in shaping government policies. (Note that although the turning point in the loyalists' campaign to influence British strategy came in December 1778 [p. 158], the basic policy that led the North ministry to Yorktown was fully developed by March 8, 1778.) The emphasis is all the more surprising since Norton correctly states at the beginning that "from the time they left their homes, they [the loyalists] were the prisoners, not the movers of events" (p. 16).

PAUL SMITH
Library of Congress

GLENN PORTER and HAROLD C. LIVESAY. *Merchants and Manufacturers: Studies in the Changing Structure of Nineteenth-Century Marketing.* Baltimore: Johns Hopkins Press. 1971. Pp. x, 257. $11.50.

The dedication of *Merchants and Manufacturers* reads in part: "This book is dedicated to [a man] whose spirit all historians might contemplate with profit: . . . Earl Weaver, who always has another pitcher ready." In the summer of 1972 Weaver's pitchers were ready, but his hitters were not. The Orioles fell to third place in the American League East and were saved the ignominy of a fourth-place finish only by the last-minute collapse of the New York Yankees. *Sic transit gloria mundi.* The story carries a message for baseball managers and maybe for historians as well—a message about balance.

This book does not have methodological balance. Written on a topic with prominent quantitative, aggregative features—the structure of nineteenth-century marketing—it does not contain a single table and asks relatively few of the large quantitative questions. The grand themes of the book are drawn from economics, but the economic analysis lacks rigor. Here we have the "old economic history" in pure form. The book might have been better had it incorporated some of the "new economic history."

This is not to say that it lacks value as it stands. Porter and Livesay are correct when they assert that prior to their work "little had

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been done in the way of historical investigation of the distribution of manufactured goods" (p. ix). This book helps to fill an important gap in American historical knowledge. Furthermore, it is a very good piece of old economic history, effectively exploiting a wide range of primary and secondary sources and providing a valuable and interesting treatment of the subject.

The wholesale merchant of the early nineteenth century was the traditional sedentary merchant dealing in a variety of generic goods. The growth in the volume of trade across the nineteenth century encouraged him to narrow the range of goods with which he dealt and to limit his marketing and financial functions: the increase in the scale of the market encouraged specialization. In the years after the Civil War technologically complex goods became an increasingly important part of manufacturing output, and since these goods were typically distributed by manufacturers, their development tended to undermine the independent wholesaler. Porter and Livesay elaborate these themes with a rich account of the distribution of manufactured goods in the nineteenth century.

ROBERT E. GALLMAN
University of North Carolina, Chapel Hill

RICHARD L. RAPSON. *Britons View America: Travel Commentary, 1860–1935*. Seattle: University of Washington Press. 1971. Pp. xi, 274. $12.00.

Although youthful America rejected its European past, it desperately needed the approbation of European visitors to these shores. Tocqueville's reflections on American democracy were pondered as much in the New World as in the Old. The negative comments of Mrs. Frances Trollope and Charles Dickens cut to the quick not only because they were largely true but also because Americans were highly sensitive to European opinion.

No longer so susceptible to criticism, Americans nevertheless have remained keenly interested in what others think of them. In 1864 Henry T. Tuckerman's *America and Her Commentators; with a Critical Sketch of Travel in the United States* represented the first of a long line of analyses of foreign perceptions of American society. More recently historians such as Allan Nevins, Henry Steele Commager, and Oscar Handlin have edited and introduced volumes of travel literature. British visitors have been of special interest to American historians. In 1922 Jane Louise Mesick produced *The English Traveler in America, 1785–1835*, and in 1943 Max Berger continued the story with *The British Traveller in America, 1836–1860*. Richard Rapson is of this lineage.

Yet Rapson is vastly different from Mesick and Berger. Whereas they usually ignored sectional differences and chronological development within the topics of their investigation, he shows how the British perception of American schools, homes, women, government, and churches was determined not only by the geographical reference but also by America's development from a "land of youth" (ca. 1860–1900) to a "land of young adulthood" (ca. 1885–1935). Moreover, Rapson employs the tools of behavioral scientists in a manner foreign to Mesick and Berger. His appendix-note on "The British Travelers as Amateur Anthropologists" is a gem for which students of travel literature can be grateful.

The annotated bibliography of over 250 travelogues for the period 1860–1935 is excellent, but contradicts the author's repeated suggestion that the South was ignored by British travelers. Granted that few famous Englishmen went south of the Mason-Dixon line after 1865; but many lesser figures did, and wrote about it. Although the West was of prime interest in the late nineteenth century, the Victorians were too concerned about racial issues to ignore the American South.

Despite all their biases and misconceptions concerning America, British travelers are a valuable source of historical information. With fresh eyes they saw many things that Americans took for granted. Their insights into familial relationships, societal values, and political assumptions are simply not available in the works of native commentators. "If some historians laugh condescendingly at the British travelers," Rapson concludes, "they do so, in part, because they themselves have misused the travelers." For a lesson in the sensitive, helpful use of these travel accounts, Rapson must be read.

WILLIAM J. BAKER
University of Maine, Orono

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DIETER BRÖTEL. *Französischer Imperialismus in Vietnam: Die koloniale Expansion und die Errichtung des Protektorates Annam-Tongking, 1880–1885.* (Beiträge zur Kolonial- und Überseegeschichte, Number 8.) [Zurich:] Atlantis. 1971. Pp. 525.

Dieter Brötel's monograph on French expansion into northern Vietnam between 1880 and 1885 developed from his doctoral dissertation at Heidelberg. It retains the virtues and vices of such studies. Very detailed, scrupulous in documentation, and admirably organized, it provides a view of an ambitious imperial power employing its strength for political, economic, and military ends. Brötel uses archival material and contemporary French accounts to the fullest extent, while his survey of the European scene, especially of the scramble for concessions, allows him to set French activity in its global context.

Brötel's reasons for French expansion are familiar: an economic depression at home, a desire not to be squeezed out of East Asia, especially by Britain, an ambitious navy, and financial interests that saw in Indochina the source of endless raw materials. Brötel's organization of these strands and his lucid narrative make for a clear if rather standard picture.

The most serious drawback in *Französischer Imperialismus in Vietnam* is the invisibility of the Vietnamese. Vietnam exists in Herr Brötel's work merely as a setting for French maneuvers. Although such a standard work as Lê Thàn Khôi's is found in his bibliography Brötel gives no impression of having used it, much less the exemplary recent studies of David Marr, Alexander Woodside, Milton Osborne, and Truong Buu Lam or the vast compendium of Joseph Buttinger. The resistance of many Vietnamese, the collaboration of still others, and the painfully reminiscent depredations of French and colonial forces are not "another story" but the heart of the matter. One of the members of the French expeditionary force, Captain Gosselin, shortly after the coronation of the French puppet emperor Dong Kanh, admitted: "The installation of a new sovereign at Hue did not produce the expected results for the pacification of the country. Far from being resigned to accomplished facts, the whole of Annam, from north to south, rose in the name of the fugitive sovereign."

The volume is handsomely produced and contains over 130 pages of notes, but is marred nearly fatally by the absence of an index.

JONATHAN MIRSKY
Dartmouth College

GERHARD BRUNN. *Deutschland und Brasilien (1889–1914).* (Lateinamerikanische Forschungen, 4.) Cologne: Böhlau Verlag. 1971. Pp. xiii, 316. DM 48.

This careful study offers a thorough examination of German-Brazilian relations during the first twenty-five years of the Brazilian Republic. Brunn concentrates on diplomatic and economic aspects, picturing Germany's policy toward Brazil as often inept, arrogant, and based on woefully unrealistic reports from her diplomats. Government attempts to promote emigration (for example, subsidies to the ill-fated *Hanseatische Kolonisationsgesellschaft*) and to create a more pro-German press in Brazil miscarried for lack of resources and, more important, because of serious misevaluation of Brazilian conditions. There was no lack of grandiose ideas for the "conquest" of southern Brazil, which some propagandists in Germany thought might become detached from the rest of the country. Although only a minority in Germany espoused such visions, Brazil fitted into the widely enunciated aim of using immigrants in concentrated overseas settlements as leverage for the promotion of German exports and political influence. Nonetheless, Brunn finds that German efforts at economic penetration were too half-hearted and too lacking in private support to qualify as a genuine case of "informal imperialism."

Especially valuable for students of Brazilian history will be the analysis of the role of the German immigrant community. Despite intermittent efforts by the German government to maintain a consciousness of *Deutschtum,* the immigrants inexorably became Brazilianized. The persistence of German-speaking schools and churches alarmed Brazilian nationalists such as Sílvio Romero, yet the fear of a "German menace"—a potentially disloyal foreign minority—proved illusory when war came in 1914.

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Brunn shows clearly how the German government faced a Brazilian hostility it could never effectively overcome because of the cultural prestige of France and the predominant economic influence of Britain, then being steadily replaced by the United States. A striking example was the failure of Baron Rio Branco, the enormously adept Brazilian foreign minister, in attempting to arrange a German mission to train the Brazilian army. Although individual officers studied in Germany, the Brazilian government was never able to substitute German influence for that of the French, who had sent a mission to train the militia of the state of São Paulo. It would have been interesting to see Kaiser Wilhelm II's officers teaching discipline in Brazil.

Although largely conventional in its conception and coverage, Brunn's monograph is an authoritative reference work based on a thorough utilization of archival sources and contemporary printed materials, primarily from the German side.

THOMAS E. SKIDMORE
University of Wisconsin, Madison

HELGE GRANFELT. *Alliances and Ententes as Political Weapons: From Bismarck's Alliance System to Present Time.* (Publications of the Fahlbeck Foundation, Number 48.) Lund: CWK Gleerup. 1970. Pp. 298.

Granfelt's major publications in German have dealt exhaustively with the alliance system comprising Germany, Austria-Hungary, and Italy from its antecedents to 1902. This work has the more comprehensive goal of reviewing the purposes of alliances and ententes during the past century. Its four parts are described as covering periods of status quo, imperialism, pacificism contra dictatorships, and democracy contra communism, and they are chronologically divided at 1890, 1914, and 1940. To each section a bibliography of primary and secondary sources is appended.

The promise of the title and organization, however, remains unkept. If Granfelt has a thesis, it may be the unexceptional point that alliances and ententes are means of reaching goals. In fact, the text is so formless that it provides neither a comprehensive analysis of its alleged topic nor a superficial review of the diplomatic history of a European-centered world. The book's contents and bibliography reveal that arguments vital to the topic, such, for instance, as those of Arno Mayer, are ignored by Granfelt. One is tempted to conclude that a major problem lies in the author's inability to communicate in English. While one may smile at such malapropisms as "Russia hoped to make an appliance of small Balkan states" (p. 20) or the description of Woodrow Wilson's adviser as "Colonial House" (p. 136), what is one to make of such all-too-typical sentences as "Benefits thus increase standard of living in undeveloped Common Market countries have followed the American methods" (pp. 218–19)? And can it be that only a passing acquaintance with the language of composition allows the author to write that "before the First World War no anti-semitism existed in Germany" (p. 145)?

The book is printed on stock of high quality.

HERBERT D. ANDREWS
Towson State College

MEYER W. WEISGAL, general editor. *The Letters and Papers of Chaim Weizmann.* Series A, *Letters.* Volume 3, *September 1903–December 1904.* New York: Oxford University Press. 1972. Pp. xliv, 414. $14.75.

Volume 3 of this meticulously edited and authoritative series covers two important events in the early history of Zionism: the so-called Uganda project and the death of Theodor Herzl. This is also the period in Weizmann's life when he settled in England and began his distinguished career in chemistry.

The identification of Zionism with the historic Holy Land and its success in establishing a Jewish state there have tended to obscure the fact that early in the twentieth century there were a number of schemes for Jewish settlement and autonomy in various parts of the world. The best remembered of these, the British offer of a territory in East Africa (today in Kenya, often then as now erroneously placed in Uganda), was tentatively endorsed by Herzl himself and by the Sixth Zionist Congress in Basel in 1903. In general the Zionists of Western Europe, whose monopoly of the leadership and lack of ideological depth Weizmann had long deplored, were in favor of East Africa as

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an immediate necessity for rescuing East European Jewry. But even in Russia, where the strength of the *Zionei-Zion* or "nay-sayer" opponents of East Africa lay, Zionist opinion was bitterly divided. Weizmann's letters reflect the pessimism but also the determination of the "nay-sayers" who were, in fact, shortly to regain control of the Zionist organization.

The breach with the "yea-sayers" or territorialists was irrevocable, and the latter soon founded their own organization. The breach with Herzl seemed scarcely less so, and only his sudden death—at which even bitter opponents like Weizmann deeply grieved—restored Herzl's image as prophet and founding father.

Weizmann in this period, though extremely active among the "nay-sayers," is not yet a leader of the first rank. But already he describes himself as "some kind of self-styled diplomat . . . to the British government." Later volumes will no doubt be of still greater interest and value to historians as Weizmann's career as a Zionist statesman unfolds.

SOLON BEINFELD
Washington University

NIEL M. JOHNSON. *George Sylvester Viereck: German-American Propagandist.* Urbana: University of Illinois Press. 1972. Pp. x, 282. $9.95.

KLAUS KIPPHAN. *Deutsche Propaganda in den Vereinigten Staaten, 1933–1941.* (Beihefte zum Jahrbuch für Amerikastudien, Number 31.) Heidelberg: Carl Winter, Universitätsverlag. 1971. Pp. 223.

These two books deal with pro-German propaganda in the United States before our entry into World War II. There the similarity ends. Niel Johnson has written a sympathetic but not uncritical biography of George Sylvester Viereck, the German-American poet and propagandist who assumed as his life's work the burden of explaining his "mother," Germany, to his "wife," America. Viereck, whom the Nazi government considered its most effective American propagandist, avoided most of the German and German-American agencies doing similar work during the 1930s partly because he believed them ineffective if not counterproductive. The organization, goals, and methods of these groups constitute the main concern of Klaus Kipphan.

Although Kipphan has worked diligently in German and American archival and secondary sources, his book adds little to our knowledge of Nazi propaganda or its influence here. The confusion and the lack of unified leadership in the propaganda effort have been documented in large part elsewhere, as has the opposition of Germany's Foreign Office to the blatant techniques of the German-American Bund. Kipphan is strongest in explaining the organization and activities of the various groups engaged in propaganda, from the Bund to the Nazi party, and the ways in which the propagandists made use of American antiwar associations and politicians. All these attempts to keep America neutral, however, were destined to fail because of a basic lack of sympathy for Nazi goals that could not be overcome by even the most clever propaganda. The Foreign Office understood the unfeasibility of presenting a positive image of German policy and devoted its low-keyed propaganda mainly to strengthening anti-British sentiments. Kipphan observes with irony that the hostility of Americans to German propaganda was disproportionate to both its efforts and effects. One can hardly blame the author for the unsuitability of the Nazi program for a large-scale propaganda campaign in America, but the fact of the program's inevitable failure creates a sense of slackness in this monograph.

Johnson, on the other hand, creates a dual tension that makes his book interesting and provocative. First is the lifelong conflict between Viereck's emotional affinity for Germany and his equally sincere American patriotism; second, the strain that emerged in the late 1930s: Viereck's individualism and his desire to act honorably seemed incompatible with an increasing need to justify acts of Nazi Germany that went against his grain. Johnson does not gloss over some of the propagandist's less savory activities during the late 1930s through 1941, nor does he excuse them. But Johnson does convince the reader of the pathos of Viereck's sense of mission—of his psychic need to believe in Germany regardless of who represented it.

One cannot, moreover, avoid the judgment that Viereck was at least as much sinned against as sinning. Even though he dropped his initial opposition to support America in World War I, the Poetry Society expelled the promis-

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ing young poet from its ranks. More seriously, Viereck suffered continuous persecution by the government during World War II and even before Pearl Harbor. Although he had registered as a foreign agent in accordance with the law, Viereck was charged and tried three times for dubious crimes ranging from sedition to concealment of some of his propaganda activities. Convicted twice, he spent almost five years in prison. Here as with Kipphan's book one questions the American overreaction to German propaganda, small in scale and largely ineffective, even before the United States entered the war.

Although Johnson astutely develops the tensions in Viereck's life, he is less successful in explaining how the propagandist reconciled them. One closes the book without really comprehending why Viereck chose to abandon his poetry, the esteem of his friends, and even members of his own family to support a cause whose methods by 1939 were out of harmony with his own standards. Johnson usually accepts Viereck's explanations of his own motives at face value. I, at least, cannot. A last minor quibble: Johnson has a tendency to label as "isolationist" all who opposed the League of Nations or the prospect of war in the late 1930s. This outmoded simplification is a disservice to men like William Borah and a flaw in a generally fine book.

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ANCIENT

RUSSELL MEIGGS. *The Athenian Empire.* New York: Oxford University Press. 1972. Pp. xvi, 620, 4 maps. $25.50.

Meiggs's study of the Delian Confederacy, that military alliance of autonomous and equal Greek city-states that the Athenians transformed into an imperial organization subject to their regulations, offers to the scholarly world the first comprehensive study of the Athenian Empire. This monumental volume will remain for decades the major study of Athenian imperialism. With a careful review of the evidence, a study of the origins of the confederacy and an analysis of its development from 478 B.C. to its demise in 405 B.C., and an analysis of later ancient judgment, Meiggs organizes the multitude of previous scholarship into an outstanding study of 412 pages and 190 pages of elaborately detailed endnotes and appendixes.

Throughout the volume Meiggs's careful scholarship is forever present, and gems of perception lead the reader on, often through material previously presented by others but now organized in a cohesive form. Of particular value are chapters 13 and 14 in which Meiggs investigates the evidence of the Athenian tribute lists and the economics of the Empire and writes a comprehensive study that surpasses the earlier surveys of confederate economics.

Nevertheless, the reader is constantly asking more of the study than time and space allowed. For example, the footnotes are sparse and tantalizing, often giving only one prime ancient source for the topic in question and rarely citing recent scholarship or the arguments for rejecting that scholarship. Within the endnotes Meiggs cites the ancient evidence and the major secondary works, yet other and all too numerous crucial questions arise to which Meiggs does not address himself. Because the imperial focus is Meiggs's concern, many national affairs of Athens, Corinth, Sparta, and even Persia are not considered fully or at all, yet these are problems of the Empire. For instance, not until the discussion of the Ionian War and the events of 412 B.C. does Meiggs squarely address his questions to the acts and motivations of the Persian kings and their empire. The Athenians structured the confederacy and created the Empire because of Persia, yet Persia's role is essentially missing. The selected bibliography fails to cite any literature from the Persian side of the question, not even A. T. Olmstead's *History of the Persian Empire* (1948), which although out of date is still valuable. Also lost in the imperial description is the affect of the evolving democracy of Athens upon its imperial motivation, for example the Ephialtic reform, in addition to a detailed discussion of the anti-imperial politics of Thucydides Melesiou.

On the other hand, Meiggs's concern with the Periclean building program does not thoroughly grapple with the archeological materials, and thus his analysis fails to consider the plans for the temple of Nike as a conservative

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program in conflict with Pericles' grandiose plans for the Parthenon, the Hephesteion, the temples at Rhamnous, Acharnai, Sounion, and the Eleusinian Telesterion as the victory of the liberals and the assembly. Meiggs's argument for the delay of the Nike construction suggests Callikrates' employment on the Parthenon but fails to consider that Callikrates was employed not on the Parthenon but to utilize the plan of the Nike temple for the temple of Artemis Agrotera.

In regard to Thurii, Meiggs also does not comment on Donald Kagan's thesis (*Outbreak of the Peloponnesian War* [1969]) that Pericles seized Thucydides Melesiou's plan for that colony, tranformed the plan into an international adventure, undercut Thucydides' political position, and then actively worked for his ostracism.

For other topics not thoroughly explored, such as the political position of Aristophanes, the student can now turn to K. J. Dover's *Aristophanic Comedy* (1972), although not thorough in itself, for a more realistic interpretation of Aristophanes and Dover's revision of the so-called anti-Cleon attitudes. Similarly, the fundamental question of the growth of stasis in Athens after the Syracusan campaign as the major cause of the failure of Athens within the Empire has now been considered more fully by W. Robert Connor in *The New Politicians of Fifth-Century Athens* (1971). I also find the discussion of the Athenian Currency Decree less than satisfactory. The numismatic problems are complex and prolific, yet a study of the literature would reveal several important patterns that led the Athenian boule to consider that legislation: the absence of Island coinage after 480 B.C. and the probable use of Athenian coinage; the subjection of allies, namely Thasos and Aegina, and the closure of their mints as a practice that limited sovereignty and economic self-determination, which, in turn, led to the decree; and other mint studies that support the mid-century date. In a similar vein Meiggs does not raise several important questions. Why did the Thracian states resume coinage? What were the numismatic and economic problems when the Athenian mint faltered and when wide-scale resumption occurred throughout the Empire? And when did economic havoc spread throughout the Aegean as a result of the Athenian government's weakened support of the Athenian token coinage?

Numerous other points also bother me, such as Meiggs's undeveloped rejection of MacDowell's suggestion of Aegina's charter membership in the confederacy, Bowersock's date of around 443 B.C. for the pamphlet by Pseudo-Xenophon, Asheri's suggested date of 438 B.C. for Brea, and many other points too numerous to mention. In short, this excellent volume addresses many questions, but also raises many.

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G. E. M. DE STE. CROIX. *The Origins of the Peloponnesian War.* Ithaca: Cornell University Press. 1972. Pp. xii, 444. $17.50.

After those dreary years marked only by one unfortunate book after another on Sparta, Athens has finally come into its own. Russell Meiggs's *The Athenian Empire* (1972) is a careful survey, though reserved on critical questions almost to the point of passing no judgment; Ste. Croix on the other hand has a powerful thesis, which he argues brilliantly and on a grand scale. The treatment is deliberately analytical rather than chronological (as in Kagan's study of the same topic).

The Athenian Empire, in sum, arose "inevitably" by the late 470s and was essentially supported by its subjects—a well-known view of the author's that has aroused a fascinating debate in *Historia* since 1954. Pericles and others earnestly sought to maintain peace with Sparta and its allies, which were "the real aggressors." More extended sections discuss Spartan foreign policy and Spartan relations with Athens. Ste. Croix's aim is to prove correct the judgment of Thucydides 1.23 that "the truest explanation, although it was least publicised, was that the Athenians becoming great and instilling fear into the Spartans compelled them to go to war."

Since I do not agree with the author's initial statement that Thucydides' study is "by far the best historical work" surviving from antiquity, the occasional effort to support a point by an appeal to Thucydides' authority leaves me unconvinced. In this respect it is intriguing to observe that on page 177 Herodotus is blamed for

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repeating slanders, while two pages later Thucydides is excused for accepting a forged document. Essentially the pictures of Sparta and Athens are warped. On Sparta, for example, Ste. Croix accepts the doubtful story in Diodorus 11.50, repeats once more the tale of Helot slaughter in Thucydides 4.80, and as usual omits the next, remarkable sentence in this passage that the Spartans sent seven hundred Helot full soldiers (hoplites) with Brasidas. Before finishing with Sparta Ste. Croix even drags in the revolt of Agis III in 331 B.C., one hundred years later. Where the author disagrees with Andrewes or Strasburger, he is not necessarily right.

The crux for strict Thucydideans is the Athenian decree that banned the Megarians from the Athenian Agora and the harbors of the empire (to use Ste. Croix's very narrow interpretation); for both in Aristophanes and in the diplomatic debates of 432–431 this apparently vindictive measure seems to have been a serious bone of contention. The lengthy chapter in which Ste. Croix seeks to reduce and even to eliminate its importance is superb in argumentation and wrongheaded in its thrust. He lists forty-two historians who have thought incorrectly; without pretending to be the forty-third let me note just two problems. If we agree that the charge of peculation against Pericles in Aristophanes, *Peace* (lines 605–18), is pure slander, that does not "disprove" the emphasis placed in this passage on the Megarian decree (pp. 236–37). Again, Ste. Croix asserts (pp. 389–90) that Plutarch, or his source, actually looked up the decree in Craterus' collection so far as to select a precise phrase, but that for the rest Plutarch's summary "all agoras and harbors" may be dismissed as inaccurate. Is this logical?

Editors really must perform appendectomies far more ruthlessly. Of the forty-seven included in this book (108 pages in total) I omit three as debatable; of the rest eight are germane, six should have been incorporated in the text, twenty-six might well have been omitted, and four could have been published separately. The work then might have cost less and would have been more easily digested either by scholars or by the general reader whom Ste. Croix hopes to attract (though one sadly suspects that any general reader who did not have the Peloponnesian War at his fingertips would have quit after the first three pages). No serious student, however, should stop so soon. The prose is clear and vigorous; the turn of argument is always provocative of thought. In his debate Ste. Croix is fully aware of old and new scholarship alike, which is summed up in a select bibliography.

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MEDIEVAL

JOHN HARVEY. *The Mediæval Architect.* New York: St. Martin's Press. 1972. Pp. 296. $17.95.

The present volume is one of two books by John Harvey covering similar material written in rapid succession. While neither is meant for scholars, *The Mediaeval Architect* is aimed at a more informed audience. It is intended for interested amateurs while the appeal of *The Master Builders* (1972) is to total laymen.

According to the jacket flap Harvey's intention is to recap our knowledge of the men responsible for architectural production in Western Europe from around 1000 to 1500. This is a reasonable objective. The proliferation of scholarship in this area would make a summary instructive to the general reader and useful for the specialist. Unfortunately the author has produced a highly subjective critique rather than an objective summary. The book is marred by romantic and simplistic interpretation, overt nationalism, and, frankly, a startling naiveté. As a result *The Mediaeval Architect* assumes dubious value for any reader. A few examples will suffice to bear out these charges.

In the preface and the introduction Harvey sets his chronological definition of the Middle Ages and explains his terminology. He finds several reasons to start at ca. 1000. He resorts to the millennial hypothesis, which he does not discuss but clearly accepts, later citing the *de rigueur* quotation from Glaber (p. 56). Further, he mentions a "movement," starting in the eleventh century, that he feels altered Western European civilization. "This movement could be of two kinds: the first is represented by the missionary enterprise of bishops and priests from the Byzantine Empire . . . who came to

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the West to preach, or to rule dioceses. To men of higher culture such as these, the crude barbarism of most of north-western Europe must have been barely tolerable. To remedy this . . . they would . . . have had to send overseas to regions of higher culture, and more advanced technical education, for artists able to improve the material conditions of their new homes, and to design and build cathedrals and churches not completely unworthy. This importation . . . produced a substantial change for the better by environmental means. To the extent that these artists may have brought their families with them, or founded new families in the West, they also raised the standards attainable in future generations by biological means. . . . The second type of movement . . . was responsible for the flowering of mediaeval culture upon the highest plane, putting it in the thirteenth and fourteenth centuries on a level with any of the most exquisite achievements of mankind. This was the arrival of biologically better blood in the families of the ruling dynasties of western Europe. . . . Just as the importation of a few steeds of the best Arab blood made possible the modern European racehorse, so the bridal journeys of princesses given in marriage to western rulers provided the mainspring for the complex machine of mediaeval European civilization" (pp. 13–14). Among other things these passages illustrate Harvey's tendency to present speculation as fact. As for the biological discussion, it is difficult to believe that it was written in 1971. Harvey's reason for accepting 1500 as a terminal date underlines his subjective interpretation of history and architectural history. "The tragedy of the Renaissance lies in its recovery of the dead written code of Vitruvius as a method of design: architecture since 1500 has been as it were commentary and case-law on the Vitruvian Code" (p. 42).

The general meaning Harvey gives to the term "architect" cannot be faulted. He accepts the broad definition that an architect "is a man who is capable of envisaging a building, complete and in detail, before one stone is laid upon another and is also capable of so conveying his vision to the actual builders that they are able to translate it into actual reality." (L. F. Salzman, *Building in England down to 1540—A Documentary History* [rev. ed.; Oxford, 1969], 4–5. Quoted by Harvey on page 12.) It is an elaboration of this definition that occupies most of Harvey's text. The medieval "architect" that emerges is a romantic composite type—a timeless ideal rather than a real figure. As has been remarked elsewhere, "The reader should be aware that Harvey has found more medieval 'architects' than any other historian by his failure to differentiate between designers, masons, and clerks of the works, and that he fails to indicate that a given practice known to be true in 1500 may well not have been so in 1100 or 1200 or 1300." (Carl F. Barnes, Jr. in his review of *The Master Builders*, in *Choice*, 9 [1972]: 636.)

This simplism carries over into other aspects of Harvey's book. For example, he reduces the differences between Romanesque and Gothic architecture to the difference between the round and the pointed arch (pp. 49, 71). Nationalism appears when Harvey dangerously underplays the role of France and the rest of Europe in the history of Gothic architecture in favor of England (pp. 156–65).

One further irritation must be noted. Harvey, a trained achitect, states that his book is not in any sense a research thesis (p. 14). Yet the book has scholarly pretensions, including an apparatus of appendixes, footnotes, and a bibliography. The footnotes are cursory and difficult to use. They are not numbered but rather keyed to words in the text. Thus the reader must check the notes at the end of the book to see if the author has cited sources. The bibliography, despite its considerable length, is inadequate. Missing are several relevant works such as those by James Ackerman, Sumner Crosby, Anselm Dimier, Jean Gimpel, George Kubler, and Robert Mark. Had they been consulted, perhaps Harvey would have felt less inclined to indulge in gratuitous criticism of recent scholarship.

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BRIAN TIERNEY. *Origins of Papal Infallibility, 1150–1350: A Study on the Concepts of Infallibility, Sovereignty, and Tradition in the Middle Ages.* (Studies in the History of Christian Thought, Volume 6.) Leiden: E. J. Brill. 1972. Pp. viii, 298. 72 gls.

More than a hundred years ago, while *Pastor aeternus* was still unpublished, Döllinger

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wrote: "To prove the dogma of Papal Infallibility from Church history nothing less is required than a complete falsification of it." Professor Tierney has returned to this theme with a bold, powerful, and original book. Like Döllinger, as a historian he knows that in the early fourteenth century the idea of papal infallibility was "blankly unacceptable" to such extreme supporters of the Roman see as Augustinus Triumphus and Alvarus Pelagius. The doctrine emerged in the course of history, and, with another century of scholarship to support him, Tierney is in a better position than was Döllinger to suggest when and why it emerged. His findings may be quoted in his own summary: "There is no convincing evidence that papal infallibility formed any part of the theological or canonical tradition of the Church before the thirteenth century; the doctrine was invented in the first place by a few dissident Franciscans because it suited their convenience to invent it; eventually, but only after much initial reluctance, it was accepted by the papacy because it suited the convenience of the popes to accept it."

Not everyone will find these conclusions comfortable, and Tierney's progress to them will be scrutinized with much care. What he offers is a historical investigation; he knows that it is not for the historian to do the theologians' work for them, though he is well aware that historians' work in this field cannot fail to evoke a theological resonance. He begins by seeking to establish his negative conclusion in an area where his earlier writings have earned him high esteem: the canon lawyers of the twelfth and thirteenth centuries, he contends—and here he parts company with Döllinger decisively—came nowhere near the modern doctrine of papal infallibility. Indefectible as they were bound to consider Christianity itself, decretists and decretalists alike held the view that a pope might err in faith and in that case could be deposed; their opinion, indispensable for a genuine theory of papal sovereignty, that any pope could recall or revise the decrees of a predecessor (*cum non habeat imperium par in parem*) rendered any strict doctrine of irreformability impossible for them. This part of Tierney's case will not be easily shaken.

What then led to the formulation of the doctrine? Tierney argues that it was the need of the Franciscan Order for an inerrant and unalterable declaration that the Franciscans' interpretation of the teaching of St. Francis and the theory of Evangelical Poverty that they built upon it were true. Such a declaration they thought they had gained from Pope Nicholas III with *Exiit qui seminat* in 1279; from that time on, Tierney maintains, members of the Franciscan Order (generally awkward or unorthodox ones) provided the driving force in developing the idea of papal infallibility. Perhaps Bonaventura was already moving in that direction, but in Tierney's view the first major medieval thinker to defend the proposition was Pietro Olivi, who could do so (or rather, needed to do so) because he also believed that a pope who erred magisterially in exercising office as head of the Church ceased to be true pope and by becoming a heretic lost all power over the Church. Precisely because Olivi was convinced that false popes would arise he felt it necessary that the decrees of true popes (like Nicholas III in *Exiit*) should be irreformable. The same Franciscan preoccupation with the immutability of *Exiit* informs Tierney's next important witness. For him the declaration in the Minorite excursus inserted in Lewis the Bavarian's Sachsenhausen appeal of May 1324 against Pope John XXII, with its stress on the pope's personal infallibility when he used the *clavis scientiae* to define truths of faith and morals, represents "a major theological breakthrough," which was the more resented by John because of his high conception of the sovereign power of individual popes. And then, with barely concealed satisfaction, Tierney flips his ace on the table. To complete the list of antipapalists who gave currency to the doctrine he produces Ockham, whose insistence (still in defence of *Exiit*) on the irreformability of a true pope's doctrinal decrees meant that "he was implicitly affirming a new doctrine of papal infallibility." The doctrine had fallen into frightfully dangerous hands. If for Ockham an authentic papal definition established immutably a truth of faith, he was bound (given the ultimate intense subjectivity of his own approach to the criteria of faith) to question the authenticity of any definition that did not rhyme with his own opinions and so almost inevitably be led to undermine the authority of the head of the institutional Church.

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Only with the Carmelite Guido Terreni, who died as bishop of Elne in 1344, Tierney argues, did the new doctrine begin to be used in favor of the pope. Though his teaching found few echoes in the fourteenth century, Terreni must be regarded as the first indisputably orthodox theologian to believe in papal infallibility in a sense at all approximating that of *Pastor aeternus*.

Tierney is formidably equipped as a disputative historian. He has not resorted much to unprinted material, though he has used in manuscript some biblical glosses of Olivi, a quodlibet of Pierre de la Palu and Terreni's commentary on Gratian. But his mastery of the printed texts becomes clear from an attentive reading of what he has made of Olivi's *Quaestio de infallibilitate* or of the complicated chapter 123 of Ockham's *Opus Nonaginta Dierum:* in both cases his exegesis is absolutely first-rate. He always writes clearly, and at times with abrasive good sense, as when he declares that the views of John XXII and Nicholas III concerning *simplex usus facti* were "starkly irreconcilable with one another," or that the ecclesiological conclusions of medieval thinkers "do not follow as necessary deductions from their metaphysical premises." Concerning only a few and perhaps not very consequential matters is he at all flabby—possibly most noticeably in his handling of Michael of Cesena and the writings too readily attributed to him. What can be questioned is the sensitivity of Tierney's historical antennae. Perhaps this is too cerebral a book to be acutely perceptive or undeviatingly charitable. To demand, as he does, that the Franciscans should have regarded Evangelical Poverty as a matter of discipline rather than theology surely does far too little justice to the strength of human beliefs. When he commits himself to the statement that "the whole theory of the irreformability of papally defined doctrine had been created for the sole purpose of demonstrating that Pope John XXII was a heretic," it is obvious that, without damage to his argument, he could have expressed himself in a fashion both less offensive and historically more revealing. What was substantially at issue was not the "heresy" of John XXII but the Franciscan position as defined in *Exiit*.

His taste for paradox and propensity to a polemical tone impair the value of some of Tierney's judgments. Ockham, about whom he is intelligent in so many ways, proves capable at last of provoking him into intemperate, almost foam-flecked conjecture. Nor has the other side always fared better. To stigmatize John XXII as "merely an irresponsible amateur as a theologian" accepts too readily the abuse of John's opponents and gives too little weight to those ten volumes of Aquinas carefully annotated in John's own hand that still survive. Must we really believe, with Tierney, that John deliberately set out to destroy the Franciscan position as ratified by *Exiit* because he thought that Franciscan doctrine implied a damaging criticism of the official hierarchical church? Was it not rather because John apprehended that the divergence between Franciscan theory and practice was producing intolerable administrative disorder, while the theory itself was based on religious and intellectual grounds of suspect value?

Such doubts and reservations, though not wholly irrelevant, can in large part be discounted as peripheral to the main argument. Tierney's book has changed the terms of the debate about the fundamental historical question that it has revived. His claim that no doctrine of papal infallibility emerged before the late thirteenth century will be contested. Maybe he has been cavalier in dismissing the attitudes toward this doctrine of Gregory VII and Aquinas with no more discussion than three footnotes afford. Some, while accepting his chronology, will be disposed to argue that much more importance should have been assigned to the influence of projects for reunion with the Greeks on the development of the doctrine. Others may be inclined to press the claims of the Dominicans, Hervaeus Natalis and Pierre de la Palu, to at least some of the discredit that Tierney imputes almost exclusively to the dissident Franciscans. But this is a book that cannot be disregarded and is not likely soon to be forgotten.

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J. J. N. PALMER. *England, France and Christendom, 1377–99*. Chapel Hill: University of North Carolina Press. 1972. Pp. 282. $9.50.

The many articles that Mr. Palmer has published during the past few years have prepared

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us for some of the conclusions he reaches in this well-written and interesting study of the Anglo-French peace negotiations that began in 1375 and continued without interruption until the truce of 1389 and beyond that until 1396 when a long-term truce concluded efforts that had finally failed to produce a peace pact. The author's major difficulty seems to have been the choice of a suitable title, for the dates of Richard II's reign do not fit the subject matter, and the significance of "Christendom" does not become clear until the end.

Palmer has given a vigorous new reading to well-known documents, rehabilitated and reinterpreted many of them, and added new material to an old subject. He may be challenged in certain of his revisions but is decidedly convincing in most of them. In the last years of Edward III an important change occurred in the English plan for reaching an eventual accommodation with France; this change was subsequently adopted by Richard and became the major point in English proposals for peace. It provided that Aquitaine would be separated from the English Crown and given to John of Gaunt, who would then surrender his lands in England. This condition was a major factor in the successful talks that almost led to a peace settlement in the spring of 1394. At that moment the Gascons revolted at the news that Gaunt would hold the duchy in perpetuity and not for life. The little-noticed Gascon revolt not only wrecked the 1394 peace conference but was the main reason for France's refusal after 1396 to discuss peace at all.

Another conclusion concerns the ideology that appeared in diplomatic letters after 1384 to justify the search for peace and that owed much to the influence of Philippe de Mézières, who argued that Anglo-French peace was desirable and necessary in order to heal the Schism and launch a crusade against the Turks. Richard pursued these aims and relented from them only in 1396 when Charles VI refused to continue negotiations for peace.

In addition to these major themes Mr. Palmer shows how the domestic turmoil of the 1380s led England first to appeasement through the leadership of Michael de la Pole, then to rash plans for war by the Appellants, and finally to the pursuit of peace when Richard assumed control of the government in 1389. Palmer has repeatedly separated Richard's acts from the traditional aura of absolutism that has surrounded them and has shown in several instances that they were the acts of a realistic ruler bent on achieving a practicable peace with France, of a ruler who was sincerely desirous of ending the Schism.

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EDWIN BREZETTE DEWINDT. *Land and People in Holywell-cum-Needingworth: Structures of Tenure and Patterns of Social Organization in an East Midlands Village, 1252–1457.* (Studies and Texts, 22.) Toronto: Pontifical Institute of Mediaeval Studies. 1972. Pp. v, 299.

Holywell and Needingworth are contiguous settlements in the southwestern corner of Huntingdonshire, separated from Cambridgeshire by the river Ouse. They made up one manor of Ramsey abbey in the Middle Ages, and the abbey's manorial records make this study possible. The author's concern, however, is not with manorial relationships but with the peasant community. His preferred questions are social rather than economic and legal: Who lived here? How long? What classes can be discerned? How much cooperation and cohesion were there? More than half the book, however, deals with "structures and patterns of tenure," and here it is difficult to separate social from economic concerns, village from manor. For a work of social history one could wish for more thorough treatment of problems of population, marriage, naming of children, and especially the small family. But Dr. DeWindt has mastered his sources and methods and presents us with a careful and useful monograph.

What it shows is, not very surprisingly, a community dominated by an elite group of families separated from others by their wealth, their antiquity, and their respectability. More surprising is that these families were generally villein families, their land being held of the manor by customary tenures, while the freemen were generally smallholders who neither enjoyed so much respect nor survived so well as the villeins. When the Black Death opened up greater opportunities for peasants, the elite families secured the greater share of such opportunities. The effect of the Black Death was

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not to topple the elite but to break down the cohesiveness of the village and to increase the individualism of the peasantry as measured by violence and trespasses. Within the limits of the study, 1252 to 1457, the village was never self-contained nor self-sufficient. Its people maintained a variety of relationships with other villages and towns, not least by emigration and immigration. The years 1360–90, for example, showed net population gains, although for the whole period from 1310 to 1457 the demographic trend (as indicated by statistics on manorial tenants) was one of decline. Famine and plague do not seem to be sufficient explanations for this decline, and control of births is hypothesized. The villagers were relatively rich and wished to keep it that way.

In a preface Professor Raftis happily promises us a series of such village studies. It may be worthwhile, therefore, to suggest that his students familiarize themselves with the important attempts at "village reconstitution" by historians of early modern England and Europe as well as colonial America. Although the later sources are much fuller, some of the questions being asked by these historians might be usefully asked of manorial accounts and court rolls. Finally, it may be too much to hope that one can write a prosopographical history of a medieval village, but a bit more biography and narrative would help to convey the fact that we are reading about the past of a particular village society rather than merely a subset of a general group, the peasantry.

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HULING E. USSERY. *Chaucer's Physician: Medicine and Literature in Fourteenth-Century England.* (A TSE Monograph. Tulane Studies in English 19.) [New Orleans:] Department of English, Tulane University. 1971. Pp. 158. $4.50.

This study, part of which has already appeared in print in somewhat different form, is of interest both to historians and to literary circles. The author has applied historical methodology by his utilization of contemporary records to determine not only the position of the physician and the status of the medical profession in Chaucer's time but also the names of physicians contemporary with Chaucer and the possible models for his physician. Of greater interest, on the other hand, to literary circles is the author's concern with such questions as whether the "real" and the "comic" "[can] be distinguished from the 'ironic' and 'satiric,' the individual from the conventional and stock," whether "*The Physician's Tale* [is] appropriate to its teller"; and its literary merit?

In working from contemporary records the author has found it possible to correct erroneous views projected by earlier Chaucerians who had proceeded from the poem to the age and in so doing merely confirmed such misconceptions as that the physician was a representative of a lay profession and that he was a charlatan. In reality his designation "doctor of physic" suggests that he was a university-trained physician and a cleric, who under fourteenth-century university regulations would have had prior training in the arts and some six or more years of medical study before receiving the license to teach and to practice. He would have been knowledgeable in astrology and its application to the practice of medicine, and especially to phlebotomy or bloodletting. He would have shared in the common belief in natural magic and in the skills required by his profession, set forth by the authorities of his time and derived from his own experience. However, unlike most university-trained physicians in the fourteenth century in Oxford and Paris, Chaucer's physician appears to have been skilled in surgery as well as medicine, a combination found more often in Italian universities, where members of the medical faculties also practiced surgery. This was specifically forbidden at Paris. There surgeons had no university connection until the mid-fifteenth century. For university statutes in this regard, reference should perhaps have been made to my *Scholarly Privileges* (1962, pp. 263–66; 320–23) and "The Faculty of Medicine at Paris: Charlatanism and Unlicensed Medical Practice in the Later Middle Ages" (*Bulletin of the History of Medicine*, 27 [1953]: 1–20).

Moreover, the author has brought forth from the documents some eighty physicians in England contemporary with Chaucer, of whom six were skilled in both medicine and surgery. Of these, possible models for Chaucer's physician are William de Holme, physician in the royal household while Chaucer was there; Geoffrey

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de Melton and John de Wyke, both of Oxford; Master Louyce de Recouchez de Luce, who served John of Gaunt as physician for some twenty-seven years; and John Middelton, who was on good terms with the apothecaries and the holder of a great number of benefices and preferments.

On the whole, this study combining historical methodology with literature is a welcome addition to the growing bibliography associated with Chaucer's physician.

PEARL KIBRE
City University of New York

FRANÇOISE HENRY. *Irish Art in the Romanesque Period (1020–1170 A.D.)*. Ithaca: Cornell University Press. 1970. Pp. xvi, 240; 112, viii plates. $15.00.

Students of early Irish art have long been in Miss Henry's debt and are now more than ever as the final volume of this trilogy appears only five years after the first. This period, from the defeat of the Vikings to the coming of the Normans, was not covered in her earlier pioneering work, *Irish Art* (1940). Miss Henry's general theme is the artistic vitality of these years; the Anglo-Norman invasion marks "the death of original artistic endeavour" (p. 25).

The sprinkling of minor mistakes detracts in no way from the value of the book. The superb plates include general views, but the emphasis is on detail—minute, exciting glimpses of work of high quality. There are some telling juxtapositions of illustrations.

The period is of great interest. Vikings were absorbed into Irish society; the native church reform movement flourished; contacts with Britain and the Continent were close; new religious orders were introduced. A welcome feature is the number of dates. Study of earlier material is bedeviled by dating difficulties, but the table of dated manuscripts and metalwork (p. 120) provides a firm chronological framework for this period. People are less shadowy than before: we know of named craftsmen and royal patrons.

The pattern of chapters is similar to the earlier volumes except that the churches earn a complete chapter. Individual chapters deal with the historical background, sites, manuscripts, metalwork, crosses, and churches. Miss Henry points to the fascinating mixture of old and new: old decorative techniques like millefiori glass and champlevé enamels and new animal and foliage motifs. The distribution map of crosses contrasts strikingly with the similar map in volume 2: emphasis now is on Leinster and Munster; Meath and Ulster are almost empty. Churches are characterized by simplicity in plan and elevation, remarkable when compared with contemporary work in England and France, but the simplicity is enlivened by exuberant carved decoration. The final chapter on contacts and influences may not win universal acceptance, but it will certainly provoke discussion.

One can look for advances in several directions. Prospects are exciting from continuing excavations in Viking Dublin and on Inis Cealtra (Co. Clare). Romanesque carved fragments of high quality were recently found reused in a building in Armagh, and a scatter of Romanesque fragments in the north suggests that the blank on the map is not a real gap. Is it too much to hope that another crosier will be found behind a cupboard in London (p. 88) or walled up in a castle (p. 97)?

This book, like its two companions, will be an endless source of information, stimulation, and pleasure.

ANN HAMLIN
University of Exeter

Millénaire monastique du Mont Saint-Michel. Volume 3, *Culte de Saint Michel et pèlerinages au Mont.* Edited by MARCEL BAUDOT. (Bibliothèque d'histoire et d'archéologie chrétiennes.) Paris: P. Lethielleux Éditeur. 1971. Pp. 526, 48 plates. 200 fr.

This is the fourth volume to appear in a series commemorating the millennial anniversary of the arrival in 966 of Benedictine monks at the Norman monastery of Mont-Saint-Michel. The first (1966) concentrated on the history and liturgy of the abbey; the second (1967) on its cultural life; the third (1967) furnished a bibliography. In honor of the patron saint the present volume is devoted to the veneration of St. Michael principally in medieval Europe. Appearing first in the pages of Scripture, St. Michael was transformed into a cult by the Copts, brought by the Byzantines to southern Italy where his chief sanctuary was at Monte Gargano, and spread throughout Europe by the

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Celtic monks. In these pages thirty-two historians, archivists, and iconographers have explored the origins of the cult, its characteristic artistic expressions, the pilgrimages to the abbey, and the diffusion of the cult throughout France and the rest of Europe. To study this last phenomenon they have chiefly employed statistics, counting churches dedicated to the archangel, places bearing his name, and the popularity of Michael as a first name as well as studies of iconography and individual sites.

While an impressive amount of data has been unearthed, the general conclusions are disappointing. Despite intensive efforts, almost every contributor insists on the provisional nature of his findings. Relying on the cooperation of archivists throughout France, one investigator tabulated the numbers of churches dedicated to the saint in present-day France with little concern for their relevance to medieval times. Characteristically portrayed as the slayer of dragons or the weigher of souls, St. Michael furnishes little of interest to iconographers. Even André Grabar is at a loss to interpret the famous bronze doors at Monte Gargano. The more interesting questions, such as the influence of pagan deities like Mercury on the cult, find little agreement among the contributors. The old clichés have been exploded: "The Archangel loved heights," wrote Henry Adams about Mont-Saint-Michel. It was also true for St. Michael's Mount in Cornwall and Michael's Rock in Kerry, Ireland, but not for the majority of the churches dedicated to the saint in the French Alps. It appears that the more the cult of St. Michael is studied, the less can be said about it with certainty. This is indeed a discouraging conclusion for our age of cooperative historical investigation.

JOHN W. BALDWIN
Johns Hopkins University

OLIVIER GUILLOT. *Le comte d'Anjou et son entourage au XI^e siècle.* Volume 1, *Étude et appendices;* volume 2, *Catalogue d'actes et index.* Paris: Éditions A. & J. Picard. 1972. Pp. xxx, 470; 355, 21 plates. 98 fr. the set.

This is one of the most important regional French studies to appear in recent years. Written in the best tradition of French scholarship, it is based on Halphen's monograph published in 1906 and makes use of wider sources and new techniques of research developed since then by J. F. Lemarignier, Jacques Boussard, Georges Duby, and others. The result is a study of absorbing interest to all serious students of early French institutions. It demands careful attention from the reader. Many conclusions depend on the exact dates of charters, the correct identification of persons, and the juridical examination of formulas. The footnotes provide an essential commentary on the evidence; long but lively, many of them are learned and persuasive short essays in interpretation. But however complex the discussion, a clear picture of social structure and historical change finally emerges.

The author finds the apogee of Angevin power under Geoffrey Martel (1040–60), when the prestige earlier attached to Carolingian office survived together with his growing domination of subjects, vassals, and fortresses, and when his favoring of the pre-Gregorian Church reform secured the cooperation of the clergy without weakening his effective control over Church appointments. Later the quasi-religious prestige of Carolingian authority was slowly eroded by a new, more spiritual wave of Church reform, leaving the count with only his feudal authority. Because the discussion is so full and impartial, readers concerned with problems of development in Normandy, Brittany, the Ile-de-France, or any of the frontier regions can immediately see the relevance of contemporary events in the territories of the count of Anjou. The most valuable recent discussion of the vicissitudes of the Bellême estates in the mid-eleventh century will be found in this book. It also explains with greater precision the changing alliances of King Henry I of France broadly outlined by Jean Dhondt and analyzes the foundations of comital power in the various Angevin domains, which played a significant part in their relations with the kings.

The catalog of comital acts from 975 to 1109 in the second volume is fuller and more critical than that of Halphen. Some 150 new acts are included, and the diplomatic analysis is more rigorous and penetrating. It would have been helpful to have had the names of all witnesses and other participants for some eighty acts that

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are unpublished, and also a few genealogical tables. Nevertheless the book is very valuable and deserves to be widely used.

MARJORIE CHIBNALL
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CATHERINE MORTON and HOPE MUNTZ, editors. *The* Carmen de Hastingae Proelio *of Guy, Bishop of Amiens.* (Oxford Medieval Texts.) New York: Oxford University Press. 1972. Pp. lxxiv, 149. $15.25.

Until very recently scholars have doubted the value of the *Carmen de Hastingae Proelio.* Most historians were persuaded by G. H. White's contention that the poem was written two generations after Hastings by someone other than its reputed author, Guy, bishop of Amiens. Recently, however, Sten Körner and Frank Barlow have provided convincing reasons to regard the *Carmen* as a contemporary work and to accept Bishop Guy as its probable author. In their introductory commentary, unusually extensive for the Medieval Texts series, Catherine Morton and Hope Muntz present further evidence for Guy's authorship and effectively demolish White's criticism. Especially valuable is their identification of Hugh, one of the alleged participants in the killing of King Harold, as a previously unknown son of Count Hugh II of Ponthieu and a nephew of Bishop Guy.

The issue of contemporaneousness is crucial to the significance of this splendidly edited text. The editors have demonstrated that the *Carmen* was written no later than 1071 and most likely in 1067. William of Poitiers's account of the battle is dependent on it, whereas William of Jumièges's treatment is far less complete. Thus the *Carmen* emerges as the best literary source for the events of 1066, and the editors have supplied the first trustworthy modern edition.

In their commentary the editors provide a shrewd analysis of the *Carmen*'s contributions to our knowledge of 1066. Guy, writing without the strong Norman bias of William of Poitiers or William of Jumièges, views the battle as a desperate affair that the Normans almost lost. Thanks to the editors' astuteness in separating rhetorical from descriptive passages, Guy's meaning becomes clearer than before. Harold's army apparently won its position on Caldbec Hill by a quick tactical maneuver that demoralized the Normans before the fighting began. Again, the *Carmen* supports C. H. Lemmon's theory of an Anglo-Saxon counterattack, presenting it as a massive charge that nearly succeeded. Such information was systematically repressed by the Norman writers.

Occasionally the editors become too speculative, as when they argue against contemporary evidence that King Edward's designation of William occurred prior to the king's accession. On this and other succession matters the judgments in Barlow's *Edward the Confessor* (1970) are to be preferred. But the commentary of Morton and Muntz is always interesting, usually convincing, and sometimes brilliant. As for the text and translation, the editors have placed all future historians of the Norman Conquest in their debt.

C. WARREN HOLLISTER
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RÉGINE PERNOUD. *La reine Blanche.* Paris: Éditions Albin Michel. 1972. Pp. 366. Cloth 33 fr., paper 24 fr.

Since the author has already published a biography of a medieval queen (*Aliènor D'Aquitaine,* 1966), she anticipated the chief difficulty that would bedevil her every step in the preparation of a life of Blanche of Castile, namely, the paucity of documentary materials. Even though one might accept Matthew Paris's estimate of Blanche as *la Reine magnifique* (p. 119), it is no easy task to breathe life into queens from the ancient and medieval past. That the author failed to do this for Blanche is reassuring. The author did not permit her imagination to supply the flesh and bones contemporary documents denied her. For this reason *La Reine Blanche* is no true biography in the accepted sense. A life of Blanche's sainted son Louis IX, even a history of France from the closing years of the reign of Philip II Augustus to 1252, the year Blanche died, would have incorporated essentially the same matter as the author offers here in this biography. Over the first hundred pages, for instance, the reader catches but fleeting glimpses of the queen.

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Nonetheless, given the important role Blanche filled as regent during the years of Louis IX's minority and while he was away on the first of his crusades, it was quite proper for the author to undertake this biography. A further justification for the study is its illumination of what the author considers the two most neglected reigns in all of French history. And why this neglect? Probably the reluctance of rationalist historians to record the eminent success achieved by those two strongly motivated Christians, Blanche and Louis, in bringing peace and justice to their country. In any event the author effectively destroys the unfavorable image the name of Blanche of Castile has traditionally evoked. She was no virago, no hard, selfish woman but rather a queen who took her responsibilities most seriously, whose statesmanship preserved France from dismemberment by a self-seeking aristocracy who feared her, and whose diplomatic skill cleared the way for the eventual absorption of Toulouse by the crown. At the same time Blanche, by example and instruction, was inculcating in her son Louis that love for justice and for the poor that would make him the most popular king France ever had.

Some readers will fault the author for her heavy reliance upon Matthew Paris, and also for a measure of Anglophobia. The author concedes, for example, that the otherwise wholly despicable King John of England possessed one virtue. He was intelligent, as were all the children of Eleanor of Aquitaine. He inherited his instability, the author declares, from his father Henry II. The author here makes a double slip. Henry was almost as French as Eleanor, and he was anything but unstable.

JOSEPH DAHMUS
Pennsylvania State University

PHILIPPE CONTAMINE. *Guerre, état et société à la fin du Moyen Âge: Études sur les armées des rois de France, 1337–1494.* (École Pratique des Hautes Études—Sorbonne. VI[e] Section: Sciences économiques et sociales. Centre de Recherches Historiques. Civilisations et sociétés 24.) Paris: Mouton. 1972. Pp. xxxviii, 757. 84 fr.

In studying the men who fought in the service of the Valois kings, M. Contamine has written both an institutional and social history of the army that was, of necessity, one of the central elements in the life of the French people in the later Middle Ages. Even by the traditionally high standards of French scholarship Contamine's study is an outstanding piece of work. He describes the army in four separate periods (1337–69, 1369–80, 1380–1445, 1445–94), presenting an orderly picture of the organization and composition of Valois military forces. Within these separate periods Contamine answers virtually every imaginable question about the recruitment, size, equipment, and support of military personnel. The depth and clarity of his presentation defies summarization and demands the attention of every student of the period. In effect, Contamine is describing the origins of the standing army in France. The period from 1445 to 1494 absorbs well over half of the book, and here we are given the early history of the army that was to prove to be one of the enduring supports of the *ancien régime*. He describes the army in its entirety and then offers an analysis of its social basis, its support, and its life-style.

Contamine employs every conceivable type of source material, and he is most anxious to avoid "les dangereuses facilités de l'"impressionisme.'" The result is a great social history of the evolution of Valois armies. At the outset of the war the Valois relied on those elements of French society that traditionally fought for the king because of obligation and/or interest. By the end of the fourteenth century, bitter experience had demonstrated that such forces were inadequate and that, worse yet, those groups the monarchy normally relied upon were losing interest in the Valois cause. Consequently the Valois came to rely upon mercenary troops from both within and without the realm, men for whom warfare was truly a profession. What is striking in this context is the continued faith in the inherent military superiority of the noblesse and the determination of the Valois to regularize the military structure in such a way that military careers would be attractive to the nobility. The significance of the reform of 1445 for Contamine is that it succeeded in reserving the profession of arms for this social elite within the new context of regularized mercenary companies. In this way the reform confirmed an evolutionary process and institutionalized an already existing social reality.

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Contamine's book is a tribute to the continued vitality of the *Annales* school and an intriguing manifestation of the renewed French interest in political history of a sort. However, the distaste for simple narrative leads to some omissions. While it is true that the renewal of aristocratic dominance in the army after 1445 is confirmation of returning social stability along traditional lines, it may also be seen as a lost opportunity for the monarchy. Might not the monarchy have used the mercenary companies in conjunction with the *francs archiers* to form a professional army that would leave the monarchy independent of the upper aristocracy? The decision to treat the period from 1445 to 1494 as a whole prevents Contamine from dealing with such questions since he treats the armies of Louis XI and Charles VII as essentially the same. Furthermore, by emphasizing the results of the reform of 1445 over the process of the reform, Contamine misses the opportunity of discussing the nature and fate of the great independent mercenary companies that for a time seemed to threaten the very structure of the monarchy. Given the success of various *condottieri* in Italy, one cannot help wishing for a more extensive explanation of their disappearance in France. Finally, we rarely see this army on campaign or in battle and have little indication of how it was used to implement the monarchy's domestic policy. This is a curious perspective, for surely one of the best ways to characterize an army is by its military experiences and its functions within the state apparatus.

It is an excellent book that makes the reader cry for more when the author is too careful a scholar to go beyond his own limits of methodology and evidence. The very shortcomings of the work confirm the fertility of the topic and the value of the treatment. We are indebted to M. Contamine for a magnificent study that transforms our understanding of the nature of both late medieval armies and states. Contamine offers us the best possible proof of Namier's remark that the social history of a nation is largely molded by the development of its armed forces, and the author does us the further service of illustrating that the reverse is also true.

PAUL SOLON
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BOYD H. HILL, JR. *Medieval Monarchy in Action: The German Empire from Henry I to Henry IV.* (Historical Problems: Studies and Documents, 15.) New York: Barnes and Noble. 1972. Pp. 251. $13.00.

The author complains rightly on page 243 that German medieval history in the mid-twentieth century has fallen into a void, but his own book is not likely to stimulate interest or enthusiasm among the students for whom it is intended. There are 106 pages of narrative introduction to the kings of Germany from Henry I to Henry IV and fifty documents. A selection of documents by themselves never is a substitute for historical research. At best a selection can serve to illustrate and deepen the historian's narrative—but having been *selected,* documents by themselves can never do more. In this book the narrative itself is gravely at fault. It is entitled *Medieval Monarchy in Action,* but the actions described are a succession of mere acts and the reader is not even left to guess the why and wherefore. It is unfortunate that the distinguished editor of the series, Professor G. R. Elton, is not in a position to provide guidance. The introduction presents the succession of the monarchs in a merely chronological fashion and veers aimlessly from the most minute statements of detail, the relevance of which is rarely clear, to generalizations that are completely indefensible: for example, "The see of Hamburg-Bremen offers a paradigm of episcopal fortunes in the eleventh century" (p. 102). Some paradigm! As they stand, the documents illustrate much that has been said in the introduction. But the introduction reads as if it had been compiled from the selected documents, rather than the other way around. One must therefore ask what the criterion of the selection of these particular documents is. There is no obvious answer, especially when one finds that the last document, for instance, which leads straight into the Investiture struggle, is not really a primary source at all but a piece of narrative by Helmold of Bosau, a historian of the later part of the twelfth century. It is also difficult to give one's consent to the author's uncritical use of either Barraclough or Thompson. Especially Thompson's interpretation of German history was heavily biased by his experience of modern American history and the conflict between federal government and state

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governments. In some other cases comments in the footnotes are by Tschan and are almost forty years old. All the same, it is good to have so many charters in English translation and the diplomatic key (pp. 108–10) is really most useful for students. One wishes, however, that the author had read E. H. Carr's *What is History* (1961) because Hill seems to have forgotten that "the pulpy part of the fruit [i.e. the interpretation] is more rewarding than the hard core" of fact (p. 4). Not that fifty documents illustrating a span of two centuries can seriously be considered a "hard core of facts."

PETER MUNZ
Victoria University of Wellington

MODERN EUROPE

HORST DENZER. *Moralphilosophie und Naturrecht bei Samuel Pufendorf: Eine geistes- und wissenschaftsgeschichtliche Untersuchung zur Geburt des Naturrechts aus der* Praktischen Philosophie. (Münchener Studien zur Politik, Number 22.) Munich: Verlag C. H. Beck. 1972. Pp. xiv, 405. DM 52.

The diversionary years of the second Great War excepted, each decade of the past half century has witnessed a noteworthy address of scholarly attention to the seventeenth-century political theorist and historian, Samuel Pufendorf. The persistent recurrence of interest argues something unusually provocative in the subject—unusual both because of the academic longevity of an admittedly second-level historical mind and because of the apparent incongruity of associating a prose as dull and long-winded as Pufendorf's with provocation of any kind—and in truth Pufendorf does have an intriguing ambiguity that has worked like an intellectual magnet upon scholars eager to demonstrate the resolvent action of their methods and encouraged to find historical support for all manner of contemporary concerns. During the tensile Weimarian twenties, Friedrich Meinecke saw in Pufendorf an early antithesis of fact and theory (*Die Idee der Staatsräson in der neueren Geschichte,* 1924). In the Nazi thirties Erik Wolf stressed the precocious element of legal positivism within the chrysalis of Pufendorf's natural-law jurisprudence (*Grosse Rechtsdenker der deutschen Geistesgeschichte,* 1939). The postwar fifties witnessed Hans Welzel's acknowledgment of Pufendorf as the rehabilitator of the natural law in its modern function of elevating individualism into an authentically liberal doctrine of culture (*Die Naturrechtslehre Samuel Pufendorfs,* 1958). My study of Pufendorf as an instance in the compromise of theory was addressed to problems raised by the politics of consensus in the early sixties (*The Politics of Discretion,* 1965).

And now from our convalescent society of the seventies we have Horst Denzer's sympathetic doctoral dissertation, which underlines the integrity of Pufendorf's natural-law doctrine when it is viewed from the perspective not so much of its origins in traditional Aristotelian political ethics ("practical philosophy")—as the subtitle stipulates—as of the persistence of this holistic tradition in the substance of the Pufendorf doctrine—as the body of Denzer's work articulates. Like his predecessors, Denzer delivers a valuable insight into Pufendorf's political theory and, therewith, into the relations of modern to classical ideas of natural law. And like his predecessors, too, he has not entirely resisted the temptation—albeit with somewhat louder protestations of academic innocence than they—to press his insight further into the beckoning Pufendorfian nebula than it can securely go.

Denzer's primary achievement undoubtedly consists in his persuasive argument for the continuing role of the Aristotelian tradition in the social content of Pufendorfian natural law through the coupling of research in general academic history with the textual analysis of the specific academic writing that was the format of Pufendorf's thinking about natural law. "Academic history" (the closest cursory approximation I can find for *Wissenschaftsgeschichte*) is a Germanic version of sociointellectual history: its object is the institutional framework of organized knowledge—the structure of schools, the system of disciplines, and the academically established canon of thinkable ideas—summed up by Denzer as "this fund of traditions," "the ever-present mental atmosphere in which alone any thinker can think at all" (p. 21). For Pufendorf living in the seventeenth century, Denzer's argument runs, Aristotelian practical philosophy dominated the rele-

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vant institutional framework—that is, the teaching and scholarly writing on ethics and politics in the German universities—and Pufendorf absorbed it into the functional matter of his natural law, through his characteristic conjunction of social morality and political obligation, through the systematic structure of his "architectonic politics," through his pragmatic subordination of constituent principles to the goal of the common safety, through the teleological implications of his socialized approach to the state of nature, and through his integration of deductive reason and observed experience. From the perspective of this academic tradition, the "modern-scientific system," which was the other component of Pufendorf's mental atmosphere and which "gives the impression of . . . ambivalence" in Pufendorf, enters his natural-law doctrine by providing a new method for ordering the Aristotelian content (pp. 239–40, 278).

There is a remarkable contrast between the precision, cogency, and sophistication of this thesis within the proper bounds of its employment and its ineffectiveness outside those bounds. As a perspective on Pufendorf—that is, as a characterization of his natural-law doctrine in terms of the conditions of its academic origin—the argument is entirely acceptable, and it helps to explain the accommodating character that has previously been remarked in his system. But there is also a curious elasticity in both the conception and the execution of Denzer's work that sporadically makes his thesis function unjustifiedly as the only valid perspective on Pufendorf. Denzer avowedly writes intellectual as well as academic history *(Geistes- und Wissenschaftsgeschichte),* and he leaves it vague in principle whether he means the combination to be a compound jointly complementary to the "history of ideas" in which he classifies previous Pufendorf research or a mixture in which Denzer's intellectual history supersedes the history of ideas and therewith the results of previous Pufendorf research. His working solution seems to be to profess the first option and to practice the second. In principle his version of intellectual history, consisting of the operations performed by thinkers upon the traditions that furnish their materials, is essentially conditioned by the academic history that is the matrix of these traditions. The actual organization of Denzer's book, however, is far looser and reverses the priority: the first part of the work, comprising two-thirds of its bulk, is an intellectual history of Pufendorf in the form of an intensive substantive analysis of Pufendorf's natural-law writings, based usually on the familiar method of *explication des textes* and with only sporadic reference to the academic tradition by which his thinking is presumably defined; there follows a briefer section that applies academic categories to this material; finally, and briefest of all, comes the discussion of the seventeenth-century academic context as such, with selective and retrospective reference to Pufendorf. This procedure runs the danger of seeming to account for more than it actually does, and it is a danger that the book does not entirely escape.

Denzer makes inappropriate applications of the academic tradition to Pufendorf on two levels: he overextends the academic natural-law format within the corpus of Pufendorf's total work and he overextends the Aristotelian component within Pufendorf's natural law. Both encroachments attest to the confusion that usually ensues when a view of the whole is itself but a part of a total intellectual situation—a confusion to which the case of Pufendorf is especially susceptible, given the comprehensiveness of the Pufendorfian version of natural law and the omnicompetent claims of the Aristotelian tradition. But the argument for the invariant centrality of natural law in Pufendorf actually proceeds from a *petitio principii* when the centrality is extended to the test cases represented by Pufendorf's constitutional, historical, and religious writings whose non-natural-law sections are excluded from discussion by definition, explicitly—in the case of history, for example—because such discussion "would go beyond the framework of this inquiry" (pp. 254–55). It is not surprising that, when only the natural-law sections are included in the discussion, only natural law is found. As for the thesis of the Aristotelian centrality within Pufendorf's natural law, resort must be had to a double standard of interpretation if Pufendorf's ambivalences are to be resolved, as Denzer tries to resolve them, in terms of it. Issues of which Pufendorf was presuma-

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bly not aware—such as theory versus practice, historical objectivity versus political partisanship, and rational versus empirical disciplines—are declared to be non-issues by virtue of the Aristotelian patterns of conjunction or subsumption, and interpretations of them as ambivalences are declared to be anachronisms. The ambivalence admitted by the thesis is the disparity between modern-scientific method and Aristotelian content, but since Pufendorf was equally unaware of this disparity (as Denzer acknowledges), it is difficult to see why it is any less anachronistic by Denzer's criterion than other analyses of what Pufendorf did rather than what he thought he did. The point is not that Denzer's ambivalence is wrong; the point is that his is not the only one that is right.

Reasons for such transgressions by a scholar usually as careful, intelligent, and knowledgeable as Denzer is may be had from the book itself. It is a study in political theory that undergoes the occupational hazards of the genre: the suppleness in handling the internal relations of its ideas is counterbalanced by the rigidity in the treatment of the external relations of these ideas. The mind of Pufendorf seems little more than a function of his natural-law doctrine, which, once its identity is worked out, is calcified into a constant factor in disregard of the historical truth that even a persistent set of ideas changes meaning with changes in the amount and kind of attention their thinker pays to them. Explicitly, moreover, Denzer finds Pufendorf's "relevance" to lie in the supraindividualistic kinship between the latter's traditional representation of "the older welfare state" and the problems of "the modern social state," which for Denzer is the dominant type of contemporary state, and in the academic kinship between Pufendorf's continuing architectonic organization of the disciplines under his general science of Aristotelianized natural law and our own current tendencies toward overcoming the specialization of the sciences (pp. 26–34). Given this view of the needs of our times we need hardly be surprised if Denzer's Pufendorf emerges as a polis-minded moderate collectivist and a paragon of interdisciplinary consistency. Welcome, Dr. Denzer, to the club.

LEONARD KRIEGER
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S. D. SKAZKIN *et al.*, editors. *Rossiia i Italiia: Iz istorii russko-ital'ianskikh kul'turnykh i obshchestvennykh otnoshenii* [Russia and Italy: On the History of Russo-Italian Cultural and Social Relations]. (Akademiia Nauk SSSR, Institut Istorii.) Moscow: Izdatel'stvo "Nauka." 1968. Pp. 461.

K. Z. KIROVA. *Russkaia revoliutsiia i Italiia, mart-oktiabr' 1917 g.* [The Russian Revolution and Italy, March–October 1917]. (Akademiia Nauk SSSR, Institut Istorii.) Moscow: Izdatel'stvo "Nauka." 1968. Pp. 322.

Rossiia i Italiia had its origins in a meeting of Soviet and Italian scholars held at Rome in May 1966. The resulting volume published two years later consists of thirty contributions of uneven length and unequal substance that may be divided into four broad categories: historical essays; biographical sketches of Russian revolutionaries who played important roles in Italian life and politics; descriptions of Soviet and Italian archival materials bearing on the topic of Russo-Italian relations; and accounts by veterans of the Italian working-class movement of the impact of the October Revolution on Italy. Such a heterogeneous collection cannot be neatly summarized in a brief review, except by saying that its very heterogeneity and wealth of information demonstrate amply that the topic is worthy of further study. The subject is not as artificial and limited as it might appear at first glance to an American historian. The histories of Russia and Italy display a striking parallelism. For example, the Italian *Carbonari* had their equivalents in the Russian Decembrists; the unification of Italy during the sixties coincided with the decade of Great Reforms in Russia; and the revolutionary wave that toppled the Russian monarchy in 1917 also dealt a mortal blow to the liberal regime in Italy and brought in its wake fascism, which may be interpreted as the response of the Italian ruling classes to the threat of bolshevism. Russia intervened directly in Italian affairs during the Napoleonic era, again in the 1820s, and yet once more during the final stages of the Second World War. Moreover, Russians and Italians were conscious of the similarities between their countries, and contacts between Russian and Italian revolutionaries in both the nineteenth and twentieth centuries have been frequent and close. Someday an enterprising and imaginative historian will tell the story of

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the Russo-Italian connection; and many of the pieces required for such a synthesis are to be found scattered throughout the contributions to this collection.

When the history of Russo-Italian relations is written, one source its author will not want to overlook is K. Z. Kirova's study of the impact of the February Revolution on Italy during the period preceding the Bolshevik seizure of power. Kirova's book exemplifies both the strengths and the weaknesses of recent Soviet historiography on Italy. Since the weaknesses are well known and have been described by me in my recent review of B. R. Lopukhov's *Fashizm i rabochee dvizhenie v Italii* (*AHR*, 76 [1971]: 525), it is perhaps more useful in this instance to dwell on the strengths. Writing from a Leninist perspective, Kirova is obliged to emphasize the close relationships existing between nations and social groups within the European capitalist system. Thus she sees connections where liberal historians would be inclined to miss or underplay them. Furthermore, she has had access to archival materials, above all the records of the Russian Ministry of Foreign Affairs, which have never before been exploited by Western or, to my knowledge, Soviet historians. The result is a rich and detailed account that moves skillfully between the worlds of diplomacy, business, parliamentary politics, and the working-class movement in both wartime Italy and revolutionary Russia. The analysis is sometimes dogmatic, sometimes distorted, sometimes frustratingly myopic, but never heavy-handed; and the point of view is stimulating and refreshing. Kirova's book reminds us to what extent the fates of the various European countries had become joined to one another by 1917. If one social system fell, all might fall. And yet the fall of one opened up possibilities for expansion of the others. Thus the ruling classes of Italy, like those of the other belligerent countries, wavered between their fear of social upheaval and their appetite for expansion, ineluctably leading their countries toward the postwar crisis to which, as Kirova judiciously concludes, the Russian Revolution contributed, but which it by no means created.

ROBERT WOHL
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WINFRIED BAUMGART. *Der Friede von Paris, 1856: Studien zum Verhältnis von Kriegführung, Politik und Friedensbewahrung.* Munich: R. Oldenbourg Verlag. 1972. Pp. 287. DM 76.

In this book Professor Baumgart provides a thorough and satisfying study of the Paris Peace Congress and its impact on international relations. Half the work covers the peace negotiations preliminary to the Congress, with special attention to French and Russian policy, while the remainder discusses the various topics and provisions of the peace settlement. Excellent maps are included.

One serious flaw is the absence of archival research except for some Austrian and German materials. On this account Baumgart misses certain points, leaves some questions unanswered, and resorts to conjecture (usually shrewdly) on some others. But this flaw, as well as an occasional repetitiousness connected with the topical organization, is more than matched by the author's command of the literature and printed sources, his clear exposition of problems and events, a skillful organization of much information, and especially a responsible revisionist approach that yields many interesting and useful insights.

Only a few of Baumgart's interpretations can even be mentioned here; in the main I found myself agreeing in substance and diverging on nuances. For example, he interprets Austria's policy correctly as European and defensive and credits her with keeping the conflict from spreading into general war. But the contention that Austria remained a neutral mediator until December 1855, and that her foreign minister Count Buol aimed above all to avoid war, is not as sound. Buol's main goal was an Austro-Western alliance to re-establish peace and the European Concert by forcing moderate terms on Russia. Austria would have gone to war for this in May 1855 had Russia, rather than the West, rejected her peace plan. Again, Baumgart sees that Palmerston more than Napoleon III wanted to redraw the map of Europe (though the author argues, surprisingly, that Napoleon was interested in the Rhine frontier at this time), and Baumgart correctly regards Britain's determination to gain military victory and destroy Russia's power as more dangerous to Europe than French ambitions. But the parallel Baumgart draws between Palmerston's

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ideas and Ludendorff's or Himmler's *Ostpolitik* is strained, and he tends to overestimate the depth and persistence of British Russophobia and the degree to which the Crimean War resulted from Anglo-Russian world rivalry. There was less ruthless egoism in British policy and more folly, inconsequence, and Whig moralism than Baumgart suggests.

These are examples of possible questions or qualifications to Baumgart's theses—an indication that they are provocative. I have only one serious disagreement. Baumgart contends that 1853–56, far from ushering in a period of international anarchy, really helped pave the way for sixty years of general peace. The wars following 1856 were mere tourneys; the *Realpolitik* of Bismarck, Cavour, and others only strengthened tendencies long existent in the European system; the European Concert survived and continued to exercise its main function. To be sure, aggressive tendencies and dangerous rivalries had existed in Europe since 1815. The point is that until 1854 a system worked to check and control them. After 1854 no rules checked Cavour and Bismarck, no Concert prevented or controlled their wars and revolutionary actions. Even in Romania, as Baumgart points out, the European Concert stood helpless before a series of illegal nationalist *faits accomplis*. It will be very interesting if Baumgart can show in his future investigations that the Concert from 1856 to 1871 was more than an idea hopelessly appealed to by Austrians and other defeated European statesmen.

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GERALD EADES BENTLEY. *The Profession of Dramatist in Shakespeare's Time, 1590-1642*. [Princeton:] Princeton University Press. 1971. Pp. ix, 329. $10.00.

GLYNNE WICKHAM. *Early English Stages, 1300 to 1660*. Volume 2, *1576 to 1660*. Part 2. New York: Columbia University Press. 1972. Pp. xii, 266. $17.50.

It is appropriate that these two books should be reviewed in this journal. Though primarily intended for specialists of the greatest period of English drama, from 1576 (the building of the first regular large theater on the outskirts of London) to 1642, both works have much to offer to the general social or economic historian of the period. The very nature of their investigation impelled both authors to familiarize themselves with much more than the careers of playwrights and actors and the architecture of theater buildings. Professor Bentley investigates the status of the profession of playwright in Shakespeare's time: the relative status of any profession in any society reflects on that society's attitudes or prejudices. He investigates how much professional dramatists were paid by companies for their work; more handsomely, it turns out, than were Elizabethan schoolmasters or curates. Plays, and sometimes playwrights and actors, were at the mercy of the official censor. Professor Bentley's book includes a clear, reliable, useful survey of the activities of the Master of the Revels from 1581 to 1642. Professor Wickham shows how not only methods of stage presentation but the very architecture and sometimes indeed the fate of theater buildings were affected by the attitude of the city government on the one hand and the court on the other. And the architecture was further affected by the changing approach of different kinds of large public entertainment. The audience for bear-baiting around 1595 was much larger than that around 1615. Both books are remarkable for their skilled analysis of and commentary on a great variety of historical documents. Often fresh insights are provided about the import of certain documents from which incorrect inferences were drawn in the past.

Professor Wickham's book attempts to describe the theaters for which the playwrights discussed by Professor Bentley wrote. The period is the first one of large-scale professionalism in English theater: professional playwrights writing for professional actors and acting companies regularly performing in "professional" buildings. But Professor Bentley's book is itself small, one of several by-products of his monumental seven-volume standard work on *The Jacobean and Caroline Stage*, while Professor Wickham's is the welcome continuation of his enormous survey of *English Stages* up to 1660. We now have part 2 of volume 2. We are promised a final volume, "a comprehensive study of play production during the whole period 1300–1660."

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Yet in a sense Wickham's subject is old, Bentley's is new. The last half century has seen a great deal of scholarly investigation of theater architecture and staging in the age of Shakespeare, Jonson, Fletcher, and Shirley, ever since E. K. Chambers's four-volume *Elizabethan Stage,* with many new discoveries and plentiful speculation. Speculation has tended to be particularly fertile and dubious about Shakespeare's Globe, from Cranford Adams's all too influential work (it is highly probable that the many popular reconstructions of the Globe based on Adams's book are mistaken), to Hotson's fantastic theory of "theater in the round," to Nagler's suggestion that a large tent or pavilion was regularly used for discoveries and even a third central entrance. Wickham seldom engages in controversy with other scholars. He lists the few facts we know about the Globe on a single page and presents the evidence that make both inner stage and use of pavilion unlikely. Yet his debt to other scholars including Professor Bentley is large. In much of his book he compiles and reinterprets. His own most important contributions are perhaps the following: (a) the thorough analysis of the effects and reactions to the Privy Council Order of 1597 for the Destruction of London's Playhouses—partly carried out; yet the Globe was erected in 1599; (b) his general study of buildings called "game houses"; (c) his stress on the fact that often not only the stage itself but also the dressing rooms (tiring houses) were not part of the frame of the theater building; (d) his thesis that after 1597 the manner of staging plays at court exercised a strong influence on the manner of staging plays in public theaters and probably also on their architecture—if not on the first Globe, then on the second Globe of 1614. The latter thesis amounts to fundamental disagreement with Chambers. But while Wickham's arguments are strong he overstates his case. Though companies depended for their very existence more and more on royal patronage, they continued to perform mainly in public, and economic considerations affecting public theaters (moreover open to the sky) were different from those governing court productions. The thirty-two plates and many sketches adorn a well-produced book.

The strangest thing about Professor Bentley's book is that it is the first one on its subject. Much has been written on theaters and professional actors, but until now scholars have for some reason remained content with the vaguest notions about professional playwrights. Who were the regular professionals, who were professional only for a few years of their career, who were amateurs? Shakespeare was a regular professional, Jonson and Middleton were not, Bentley tells us with the usual thorough sifting of evidence and persuasive historical scholarship we expect from him. But what is perhaps most important for us to know is how professional dramatists were under regular contract to acting companies, usually producing two plays per year, which they then read to the actors for their approval or criticism (or rejection); how once they were paid, quite handsomely, for their play it became the complete and sole property of the acting company. Professional playwrights thus seldom had any control over possible publication, or even the selling of their play to a different company, or additions and revisions. All this, and the low status of the professional playwright, serve to explain why we know so little about Shakespeare the man, and why in such a high proportion of contemporary allusions to him, either his name is merely listed among a group of "playwrights," or his "sugared" sonnets are recommended.

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DAVID MATHEW. *Lady Jane Grey: The Setting of the Reign.* London: Eyre Methuen; distrib. by Barnes and Noble, New York. 1972. Pp. 191. $14.00.

This book, written as a sequel to *The Courtiers of Henry VIII* (1970), focuses on the leading personalities of the reign of Edward VI, particularly the young king, Lady Jane, Northumberland, and Somerset. Mathew has enriched his account with careful genealogical studies and has given attention to the homes and physical environment in which his characters lived. Lady Jane emerges as "the key figure of all this period." She is seen as a "strange figure in an English setting, a true Renaissance princess." Northumberland, heading the cast of

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supporting characters, appears as a *condottiere* who combined, like Wallenstein, a great military capacity with a desire for landed wealth. King Edward is coldhearted and solitary. Mathew rates the king's genius lower than did Hester Chapman but accepts W. K. Jordan's view that Edward was the guiding force behind the plan to disinherit his sisters in favor of Lady Jane. Most reduced in stature is Protector Somerset, who fails both as a soldier and a statesman. Running through the essay are themes of personal loneliness and provincial isolation. Mathew emphasizes that neither Lady Jane, Lady Elizabeth, nor King Edward traveled beyond the shores of England; moreover, their learning consisted of linguistic exercises pursued without historical understanding.

The serious student of mid-Tudor England will find this impressionistic account at once stimulating and baffling. Mathew believes, for example, that many courtiers were indifferent to the religious controversies of the Reformation. Sir William Petre, secretary of state to King Edward, willingly accepted whatever form of religion the Crown promoted, while Northumberland, brought up at the court of Henry VIII, similarly supported the sovereign's wishes without question. The author contends that the Henrician religious outlook survived after 1547 and influenced the "great majority of the leaders who would come in time to support the Lady Mary." These leaders, obedient to the Crown, welcomed the restoration of "King Henry's mass" but found the "idea of Rome" quite uncongenial. What is troubling is Mathew's failure in some instances to reveal the sources upon which his judgments are based. He says a great deal about Northumberland but does not appear to have read the duke's unpublished correspondence. Although Northumberland was not a prolific writer, his letters are vital for understanding his political and religious views. We are told that very little is known of Sir William Paget when in fact nearly one hundred letters survive for the reigns of Edward VI and Mary. Mathew's conclusions are then valuable primarily as guidelines for further research. On the other hand, the general reader will find this book a fascinating character study of those who surrounded Lady Jane, that idol of Protestantism who did so little but inspired so much historical writing.

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A. L. ROWSE. *The Elizabethan Renaissance: The Cultural Achievement.* New York: Charles Scribner's Sons. 1972. Pp. xiv, 412. $12.50.

In this, the concluding volume of Professor Rowse's survey of the Elizabethan age, one senses that he has arrived in the happy hunting ground for which the earlier volumes were a preparation. His journey's end is no small personal achievement and sums up admirably his overview of the special world that has been for so long his chosen residence. One can forgive at this date his long and one-sided duel with the professors of literature, his chatty and sometimes catty allusions. They have become a fixture of his style that by expectancy would be missed. We can see them now as an exuberant charge at windmills in his defense of the isle and the island-empress, Elizabeth. His larger purpose, carried off with éclat, is to relive the age, and few will care to measure by inches the miles of territory the book encompasses. Its true measure is the hitherto standard work *Shakespeare's England* (1917), in which various aspects of Elizabethan culture were written up by specialists, each writing independently of the others. Professor Rowse by contrast has anticipated what present-day historians recognize, the seamless garment of that cultural phenomenon which made the 1580s and 1590s an exciting time to be alive. He has accomplished an impossible task, and the wonder is that it could be done at all.

Yet, at second thought, the very fact of single authorship of such a work imposes its own limitations. It is necessary to ask, and Professor Rowse never pauses in his journey to define, the boundaries of culture. He repeatedly calls our attention to the elitist group who composed it; the remainder of Elizabethan society are thereby consigned to oblivion. But a social history that intends to be comprehensive must include that great nameless majority who did not write books, compose music, paint pictures, or stuff snow into chickens. It is all very well to be given the grand tour of the luxuriance of the age, the cloud-capped towers, the gorgeous palaces, the solemn temples, the great globe it-

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self; but how shall we learn of the faceless ones who brought those towers, palaces, and temples into being but who were nonetheless essential to that gorgeous result? For this patient, vermiculous exploration we need a Joan Thirsk or a Geoffrey Elton.

Yet again it is proper to remember, not to their shame, that the names Professor Rowse holds responsible for the cultural achievement—Bacon, Raleigh, Donne (rather than Jewel, Whitgift, Cartwright, or Father Parsons), Hooker, Marlowe, Jonson, Harriot, Gilbert, Harvey, and Shakespeare—were for the most part a self-made aristocracy. Professor Rowse detests Puritans (Milton occasionally excepted): the author's sympathies are plainly with the establishment. Shakespeare is admired as a prudent, conformist countryman, "too wise . . . to upset the natural arrangements of society for hypothetical gain and evident illusions," scornful (with Raleigh) of the rank-scented many. But is is worth observing that Professor Rowse's final assessment of the impress of Elizabethan culture on Europe centers almost wholly on that same upstart crow who wrote primarily to and for a popular audience.

In brief, *The Elizabethan Renaissance* is a personal record, as it must and perhaps should be—like that of Burckhardt and Huizinga to whom he dedicates the book; which is not to say that it is any less valuable as a skillful and eloquent fusion of the elements Professor Rowse sees as composing England's greatest moment.

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GEORGE MALCOLM THOMSON. *Sir Francis Drake.* New York: William Morrow and Company. 1972. Pp. x, 358. $10.00.

A fair-minded reviewer should try to discover an author's purpose and then report on his success in the endeavor. George M. Thomson in *Sir Francis Drake* does not pretend to reveal new facts about the great Elizabethan sea rover. The author's purpose is to retell the life story of the best-remembered of Queen Elizabeth's adventurers in terms that will interest the general reader. In that effort he has succeeded. This is a swashbuckling story told with a verve and spirit that would have pleased its subject. It is not a book for specialists but one that almost anyone can read with pleasure. The author writes with skill and tells his story well. He has utilized the best of secondary authorities and made an intelligent synthesis of their findings.

Specialists may quarrel with some of his generalizations. Occasionally he slips into a twentieth-century interpretation of the sixteenth century, as, for example, in referring to the "appalling manners" of Queen Elizabeth. In his discussion of Drake's ethics, Thomson is inclined to apply a modern definition of piracy to Drake's deeds. In general, however, he tries to write from the point of view of the age. He does not fall into Froude's error of interpreting Queen Elizabeth's parsimony in making money available to the navy as shortsighted avarice. He is careful to emphasize the queen's difficulties in raising money and the poverty of the royal treasury.

One of the best portions of Thomson's book is his description of the fight with the Armada. He does not repeat the popular mistake of calling it a great "victory" for the English navy but properly evaluates the various factors contributing to the Spanish disaster. Following Mattingly, Thomson gives the Spanish commander, the duke of Medina-Sidonia, better marks than he usually gets, though the author somewhat underrates the character and capacity of the duke's master, Philip II. Thomson adds flavor to his narrative with numerous quotations from contemporaries, but these are usually undocumented, and the reader is left wondering about their accuracy and sometimes about their sources.

Although students of the Elizabethan period will learn little that they do not already know, they will find Thomson's book a stirring and entertaining narrative. They may also envy his capacity to write in a vivid prose style without lapsing into flamboyance.

LOUIS B. WRIGHT
National Geographic Society

GEORGE MACDONALD FRASER. *The Steel Bonnets.* New York: Alfred A. Knopf. 1972. Pp. 395, xiv. $8.95.

This is not only a work of first-rate historical popularization but an innovative piece of scholarship which deals with a topic that has been strangely underexamined in British histo-

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riography since the eighteenth century. Much of what has been written about Anglo-Scottish border history has either dealt with that subject as an administrative problem involving the relations of the two British kingdoms in the generation or two before 1603 or as a final romantic moment in the long history of warfare between England and Scotland. In this instance the author has sought to describe the history of the borders not simply in the context of administrative and diplomatic history but as a phenomenon of social and political disorder resulting from the failure of the central authorities in Edinburgh and London to create a stable system of governance on the frontier. The consequence of this failure, which had its beginnings in the disordered circumstances made possible by the long Scottish war of independence, was an almost permanent chaos in the northern counties of England; for Scotland, where the crown's writ scarcely ran beyond Fife and the Lothians, the result was to abort all hopes of strong central government and social stability for generations. The country thus became, down to the seventeenth century, a kind of Corsica of the north. Like southern Corsica this northern region has been romanticized, its vendettas seen as noble struggles for freedom, its brigands as national heroes, particularly in the writings of Sir Walter Scott. What Fraser has done with great success, despite some arguable generalizations and genealogical allusions, is to reveal the cruel world of border society as it was—harsh, insecure, unstable, a veritable region of banditti. From time to time men of stronger stamp strove to keep the peace, but with few exceptions and for relatively brief periods the result was always failure until the union of the crowns.

The end of border lawlessness came with almost dramatic suddenness at the accession of James VI of Scotland to the English throne in 1603. Perhaps nowhere do we have clearer evidence of James's authoritarian tendencies than in the ruthless policy that suppressed border anarchy. On the other hand, as the author points out, the policy for all of its harshness must be seen in context. James was intent on creating a unified Britain by extending to the borders the peace and security that obtained in more settled regions of his kingdoms. Harsh and cruel he may have been, but he was not ignorant of border conditions and was convinced that only the most drastic of measures could pacify the ancient frontier. The result was a campaign of suppression that lasted through a single "murderous decade" at the beginning of the seventeenth century. Questionable legal tactics extending even to the limited use of genocide, and in the case of one great family, the Grahams, to mass exile, finally broke the rebellious borderers' will to resist. Though there were to be flare-ups of turbulence from time to time throughout the following century, the old terror and uncertainty were gone and the "Middle Shires" in the words of Scottish Lord Chancellor Dunfermline had become "as quiet as any part in any civil kingdom in Christianity." And so they have remained until this day.

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ALFRED PLUMMER. *The London Weavers' Company, 1600–1970*. Boston: Routledge and Kegan Paul. 1972. Pp. xviii, 476. $26.75.

Complementing an earlier volume covering the medieval and Tudor periods, this study takes the history of the London Weavers' Company from its ascendancy and decay as a trade gild down to the present. Altogether Dr. Plummer has produced an extremely thorough and well-constructed history. Using the company's comprehensive records he closely examines its seventeenth-century organizational aspects: internal structure, trade controls, income sources, civic duties, and charitable devices. These topics are interspersed with other chapters on the company's interaction with external pressures arising from changes in politics, fashion, and technology: the arrival of immigrant Continental weavers, especially Huguenots; the appearance of engine looms and stocking looms; and the importation of Asian silks and calicoes, to combat which the company hired Daniel Defoe as propagandist. The effects of events peculiar to London's history, like the Great Plague, the Great Fire, and state ceremonial occasions, also receive attention.

Some significant insights emerge from Dr. Plummer's admirable statistical analyses of the gild's activities. For example, besides showing a progressive drop in the number of apprentices bound annually between the 1660s and 1780s, the statistical analyses demonstrate a

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ninety-six per cent reduction in the percentage of apprentices coming from outside London over the same period—reflecting the quickening pace of industrialization in the provinces. Scrutiny of the social and economic origins of the boy (and a few girl) apprentices discloses a growing number from families in the trade, suggesting that by the late eighteenth century the weavers had "turned in upon themselves" (p. 85). Decline accelerated after the company abandoned its annual "search of the craft" in 1736, thus allowing interlopers to set themselves up with impunity.

In decline the company slowly extended its charitable functions to include assistance for some of the technical education ventures promoted in England during the late nineteenth century. Since the author avoids a discussion of the company's modern role, either by reference to is current (1937) bylaws (which he does not print either) or by any analysis of its recent financial operations, the reader is left wondering whether the company is an anachronistic charitable society, a developing educational foundation, an exclusive social club, or a mixture of all three.

From a wide knowledge of contemporary and modern sources Dr. Plummer enlivens his organizational and quantitative frameworks with rich, though sometimes excessive, supporting evidence and minutiae. His volume forms an important and interesting addition to the historical literature of both the textile industry and the London craft gilds.

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Merrimack Valley Textile Museum

QUENTIN BONE. *Henrietta Maria: Queen of the Cavaliers.* Urbana: University of Illinois Press. 1972. Pp. x, 287. $10.00.

Royal biography, particularly of queens, is a tricky and often hazardous subject. Professor Bone's solution is the political biography, an attempt to estimate Henrietta Maria's "significance in English politics," thus avoiding the turbid waters of the life-and-times, on the one hand, and the alternatives of scandalmongering or monarchist adulation that have often been the staple fare of more personal biographies. The result is a somewhat more thorough account of Henrietta's "political" career than can be found in earlier biographies, including that of Carola Oman, the best of them. Nevertheless the book misses its mark, and it is hard to see that it adds much to our understanding of seventeenth-century English history.

Part of the trouble lies in Bone's rather limited conception of "political influence." He successfully rebuts old charges that the queen exerted a malignant influence on her husband and that her advice unwittingly led him to the block. Charles, the author shows, often ignored her wishes; when her policies coincided with his, as in the Franco-Irish-Scottish alliance schemes during and after the civil war, Charles would have pursued them in any case. This is all very well, but it tells us little about the queen's real influence. Scattered through the book but never brought together as a coherent interpretation are the materials for a real assessment of Henrietta's significance, though one that would compel Bone to find another title. Henrietta was queen not of the cavaliers but of a more restricted court faction—Jermyn, the elder Goring and their like—that originated in the queen's household of the 1630s and whose policies the vast majority of constitutional royalists consistently abhorred. Averting the war by an army coup or winning it by reliance on an invading French, Irish, or Scottish army would in the end have destroyed the very cause—that of the traditional legal constitution—for which the real cavaliers took up arms. Bone is intermittently aware of this but provides no real analysis of the very group through whom Henrietta's political influence was exerted.

Bone makes use of French as well as English sources. But his book is based on a naive, uncritical acceptance of old-fashioned Whig assumptions and shows no acquaintance with recent secondary literature beyond the works of C. V. Wedgwood. The definitive study of the leader of what came to be called the "Louvre party" remains to be written.

DAVID UNDERDOWN
Brown University

MICHAEL FOSS. *The Age of Patronage: The Arts in England, 1660–1750.* Ithaca: Cornell University Press. 1972. Pp. ix, 234. $8.50.

The Age of Patronage by Michael Foss, with its sixteen pages of illustrations and attractive

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format, is a handsome volume, but in many ways is disappointing. The title is misleading, for there is little new about the complicated problems of patronage. The subtitle, *The Arts in England, 1660–1750,* gives a more accurate description, but even in these areas the reader who is familiar with eighteenth-century literature and the arts will find little that is new. Foss has obviously ranged widely in his reading, though he passes on what he has found in a somewhat slapdash manner. In his discussion of patronage he uses the old authorities like Alexandre Beljame and Arthur Simons Collins, apparently unaware of other important scholarship on this subject. He has nothing original to say about the difficult relationship of Samuel Johnson and Lord Chesterfield, which began well within the stated limits of his study. Indeed, Chesterfield is mentioned only once, and then only in a quotation of a remark about architecture. Yet Chesterfield surely is a key figure for any thorough evaluation of the patronage system in the early eighteenth century.

Foss brings togehter a great mass of miscellaneous material from older secondary sources, and from well-known Restoration and eighteenth-century works concerning authors, artists, political figures, and noble patrons, which he presents in nine chapters entitled "Prologue," "Court Culture," "Trimming," "The Public," "Rules and Reform," "New Prospects," "Politics and the State," "The Power of the People," and "Failure." In the notes little reference is made to recent scholarship on the figures he discusses. Often he uses an old biography and ignores completely the accepted, authoritative life. Nor is significant use made of manuscript material, though much remains to be done in areas concerned with the relationship of authors and publishers, painters and noble patrons, musicians and managers, which a thorough search through diaries and unpublished correspondences might illumine. Yet this is not to say that the volume is wholly lacking in interest. For casual dipping it can be very entertaining, reminding readers of many facts they may have forgotten and recounting stories and incidents that they may never have happened to come across. Although not an important addition to our knowledge of the period the book will be welcomed by readers who are not looking for something up-to-date or scholarly.

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H. T. SWEDENBERG, JR., editor. *England in the Restoration and Early Eighteenth Century: Essays on Culture and Society.* (Published under the auspices of the William Andrews Clark Memorial Library, University of California, Los Angeles. Publications of the 17th and 18th Centuries Studies Group, UCLA, Number 2.) Berkeley and Los Angeles: University of California Press. 1972. Pp. xi, 231. $10.00.

This is a collection of nine essays originally presented as papers in a series of seminars given in 1969–70 at the William Andrews Clark Memorial Library on the general theme indicated in the title. Seven of the authors are scholars of English literature, one is a professor of medical history, and one is art curator of the Huntington. The focus on "culture and society" is therefore largely a literary one, and it is this fact that provides the volume with a kind of unity. Collections of essays like this are often charged rather testily by reviewers for failure to possess the coherence that a more narrowly defined theme might invite. But I for one welcome generosity of scope, particularly for the period from 1660 to 1800. The comfortable old label of neoclassicism (and others like it) with its monolithic and monadic suggestions will no longer do; the period is increasingly being recognized for its experimental and individualistic energies, and what better way to recognize this than to give inquisitive and responsible scholars a liberal mandate?

With this said, what do we have here? Three essays are concerned essentially with individuals: Robert M. Adams with the world and the Church as Swift saw them (with pain and bitterness) at the time of the *Tale of a Tub,* Robert R. Wark with Hogarth's narrative methods and the difficulty Hogarth had in accommodating his expressive ideas to his rococo techniques, and James M. Osborn with Pope's friendship with the third earl of Burlington and his countess. Besides including some hitherto unpublished verses, possibly by Pope, and providing us with a delightfully circumstantial account of the friendship, Osborn defines the

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resonance of *amicitia* and shared concerns that brought Pope and the Burlingtons together. Seldom has the peculiarly rich cultural cosmopolitanism of the early eighteenth century been so well suggested.

Of the essays of wider scope I found those by James William Johnson, Jean H. Hagstrum, and John Loftis of particular interest. Johnson gives a good shake to the old assumption that this was an age in which classical knowledge was widespread and taken for granted. Although casting a very wide net, he nevertheless is convincing in his conclusions that while knowledge was meager, influence was impressive: John Bull may have been ignorant of Aristotle and Plato, but his common sense, defined and guided by a small elite, had its foundation in their thought. Hagstrum continues his interest in the relationships of the visual and the verbal, this time by examining caricature in the poems of Dryden, Pope, and Swift. His careful distinctions between emblematic and portrait caricatures bear heavily upon larger problems of intention and effect and cannot fail to be of value to readers still uncertain about just where the power of caricature resides. But alas, we must lament with Hagstrum the loss of our ability to write satire in verse with the limning eye of Pope. John Loftis considers the value and reliability of the drama as historical record—a formidable subject of inquiry. He compares the dramatists' world with that offered by the dramatists' own contemporaries and by modern historians, especially Peter Laslett and his fellow demographers; and he is careful to take account of the political and social assumptions of the dramatists and of the theoretical conventions that governed their work, in particular the Aristotelian paradox, so influential in the period, that poetry has more truth in it than history. Without summarizing all his conclusions, I would point only to his comment on the historical value of Fielding's plays. After making allowances for Fielding's biases and for the lack of quantifiable evidence in the plays, Loftis nevertheless considers them as vital records of "the emotional dimension of sociological fact." His essay is a nice illustration of the responsible way to get at this dimension.

In other essays, Maximillian Novak considers the audience of eighteenth-century fiction; C. D. O'Malley the training, skills, and social roles of physicians; and Bertrand H. Bronson the "trough of the wave," that is, the poetry of the period between Pope and Wordsworth. In this last, Bronson, with his usual grace and learning, speculates about the popularity of the Georgic and suggests that the freedom and expansiveness of the form is what made it attractive. Far from finding the past a burden (*vide* W. J. Bate, *The Burden of the Past* [1970] the writer of Georgics was fired by the form. Professor Bronson's eloquent appeal to re-examine these long, Virgil-inspired poems of the eighteenth century deserves our fullest attention. Printed as the final essay in this collection, it underlines the need for more seminars of the sort here represented.

JOHN H. MIDDENDORF
Columbia University

RUTH JORDAN. *Sophie Dorothea*. New York: George Braziller. 1972. Pp. xii, 292. $7.95.

Nearly every account of the accession of the Hanoverians to the English throne mentions the two mistresses George I brought with him to England—Kielmansegg, who was short and fat, and also his half-sister Schulenberg, who was tall and thin—but rarely is more than passing reference made to Sophie Dorothea, the divorced wife he had for twenty years kept prisoner at Ahlden in the territory of Celle. Ruth Jordan here recounts the tale of unhappiness, vindictiveness, and dynastic intrigue that brought the electoral princess who would have been queen of England to this sad state. It centers on Sophie Dorothea's escape from a miserable marriage into the arms of Count Königsmark; Königsmark's disappearance and presumed murder; and the divorce from George Louis on grounds of willful desertion, which, ironically, the princess consented to in order to gain her freedom. Though she was declared the guilty party and forbidden to remarry, nothing in the verdict of the court convened to hear the electoral prince's petition called for her confinement. She remained a prisoner because her father-in-law, the elector of Hanover, with the consent of her father, the elector's elder brother, wished it. And when

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George Louis succeeded to the electorate and then to the English throne, he saw no reason to free his ex-wife. Her fortune had been made over to him at their marriage, and he feared she might seek to reclaim her inheritance. Furthermore, had she chosen to resume her own investigations into the matter of Königsmark's disappearance, the consequences could only have been embarrassing to Hanover. The interests of the dynasty were best served by continuing her confinement.

Sophie Dorothea was not blameless, but the treatment accorded her by her family does them no credit at all. Greed, pride of dynasty, petty court jealousies conspired to bring about her ruin. It is perhaps surprising that George's English subjects did not take more notice of her fate. But she belonged to George's Hanoverian past. To English eyes the remoteness of Hanover made less terrible the remoteness of the castle-prison at Ahlden.

SHEILA BIDDLE
Columbia University

IAN SIMPSON ROSS. *Lord Kames and the Scotland of His Day.* New York: Oxford University Press. 1972. Pp. xiv, 420. $20.50.

David Masson wrote an enormous *Life of Milton, Narrated in Connection with the Political, Literary, and Ecclesiastical History of His Time* (1859–80); Mr. Ross seems to have wished to do the same for Henry Home (1669–1782), who later took the judicial title of Lord Kames. Mr. Ross pads out the events of Kames's life as a jurist and writer with large chunks of "background" of (mostly) Scottish social, cultural, and literary life in the eighteenth century. If this were cleverly written, if Ross had interesting "new light" to shed on this background, if, above all, he were able to convince us that Kames is inherently worth this red-carpet treatment, the book might have been very successful. As it is, we learn little about eighteenth-century Scottish life that we did not already know from Alexander Carlyle and Boswell's *Journals.* Mr. Ross draws heavily on the latter, even devoting one of his chapters to the problem "Boswell in Search of a Father? Or a Subject?"—did Boswell attach himself to Johnson because Boswell wanted to write a biography or because his psyche stood in need of a father figure? A fascinating question; but what has it to do with Kames?

Mr. Ross carefully traces Kames's genealogy (all the Homes and Humes seem to have been related: Henry, David the philosopher, perhaps John the playwright, down to the former fourteenth earl, now foreign secretary). Ross describes Kames's education and career as a lawyer and judge and devotes a long chapter to each of his four substantial books, *Essays on the Principles of Morality and Natural Religion, Principles of Equity, Elements of Criticism,* and *Sketches of the History of Man.* Of these, only the *Elements* still holds interest, and that only for the specialist in esthetic and critical theory—it is a pioneering attempt at a psychological (associationist) approach, and Mr. Ross properly points to I. A. Richards as Kames's modern successor. The *Principles of Equity* is a long-superseded legal textbook, and the other two are feeble speculative essays on theological, ethical, and anthropological matters.

Possibly the book's greatest value is in its detailed account of Kames's career as a jurist and the workings of the Scottish legal system—though even there Boswell has taught us much. (Why quote a two-page-long opinion Johnson once gave to Boswell on one of Boswell's law cases?) As a source book for contemporary history it contains enough errors to make one wonder about its reliability. Dean Tucker, the economist, was named "Josiah," not "Joseph" (p. 184). The Prince of Wales, among whose circle James Oswald tried to drum up interest in David Hume's *Treatise* (p. 80), was not, as the index maintains, the future George III. The secretary of state's signature on the patent appointing Kames a judge is given (p. 117) as "Solus Newcastle." Read, as in thousands of other documents, "Holles."

DONALD GREENE
University of Southern California

RICHARD B. SCHWARTZ. *Samuel Johnson and the New Science.* Madison: University of Wisconsin Press. 1971. Pp. x, 188. $10.00.

It is both valuable and useful to know that Samuel Johnson, far from being antagonistic toward the new science, applauded its legitimate successes and subscribed to what Schwartz calls its "ideology." To be sure, Johnson was

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no scientist; he probably never even read Newton's *Principia* or *Opticks* with any dedication. What Johnson knew about science he culled from its popularizers, primarily from late seventeenth- and early eighteenth-century divines, John Wilkins, Thomas Burnet, John Ray, and the famous Boyle lecturers, Richard Bentley and Samuel Clarke, among others.

To document his case Schwartz offers his own subtle and oftentimes perceptive reading of some of Johnson's better-known essays from the *Rambler* as well as of Johnson's lesser-known works such as the *Life of Boerhaave*. In addition Schwartz makes use of texts only recently attributed to Johnson and furnishes us with another example of the all-pervasive influence of scientific knowledge and methodology on eighteenth-century thinkers.

Yet the precise nature of that influence still eludes us, and Schwartz's use of the term scientific "ideology" offers only slight consolation. By it he means Johnson's adulation of an empirical, cautious methodology, a Baconianism dedicated to the humble search for simple truth founded on experience and itself devoid of prejudices, presuppositions, or special interests. Schwartz assumes that this, after all, was what the new science, as developed by Boyle, Barrow, and the Newtonians, was all about, and that Johnson gave his assent to a discipline and methodology that was above party, church, or social ideology. Yet a careful reading of those popularizers of science whom Johnson read and admired reveals that their vision of science and its uses was intimately tied to their notions of social and political stability and enlightened self-interest. Their abhorrence of materialistic philosophies of nature, which Johnson shared, rested on their fears for the survival of a political and religious order that served special interests in both church and state. It seems at least probable, therefore, that Johnson's adulation of the new science as explained by the Boyle lecturers and others was integrated, to some extent, with his understanding of social and political issues.

The only deficiency in Schwartz's account of Samuel Johnson and the new science lies in the narrow definition accorded to scientific ideology and the resulting failure to integrate it with Johnson's larger view of circumstances and events. Yet that deficiency is on the whole minor. Schwartz has rescued Johnson from a prevalent misapprehension that he had little appreciation for the new science and in the process has made a significant contribution to eighteenth-century literary studies.

M. C. JACOB
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JOHN W. DERRY. *Charles James Fox*. New York: St. Martin's Press. 1972. Pp. 454. $12.50.

In the historical writing of Victorian liberals in Britain, Fox appears as the source of their tradition and their hero. With courage, persistence, and eloquence he stood out against the influence and duplicity of the monarch, against war in union with despots against the French people struggling to keep their freedom, and with an especial nobility against repression at home. With the odds heavily weighted against him he fought for parliamentary reform, intellectual freedom, Catholic Emancipation, and justice for the Protestant Dissenters. A crowning glory of the last of his brief periods of office was the abolition of the slave trade in the British Dominions.

Dr. Derry's workmanlike survey of Fox's life corrects, in the light of more modern studies of the eighteenth century, the Victorian misunderstanding. This survey is founded on a careful study of the printed sources and of the Holland and Fox papers in the British Museum and the Grey papers at Durham. For Dr. Derry, Fox is better understood in eighteenth- than in nineteenth-century terms. Much in the early lives of both Fox and Pitt explains the antagonism between them. With at times remarkable vindictiveness they carried on the quarrel of their fathers. In education and in temperament Fox was convivial; Pitt, except among close friends, stiff and reserved. Fox was impetuous in speech and rash in action, much of a gambler not only at Brooks' club, but in other aspects of life; Pitt was cool and calculating, careful and reasoned in speech. Fox was often a bungler, Pitt a clever tactician. Both were astonishingly well read, both of them of great ability. As Dr. Derry remarks, Fox's gifts suited him for office. Perhaps it was because he was so much in opposition that he lacked discipline.

Fox bequeathed to his disciples a doctrine

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about party as the best corrective of undue court influence, a doctrine he had learned from Burke, and a version of Whiggism that Burke rejected. A corollary of Fox's party doctrine was the idea of cabinet unity, rather than a ministry of departments. His novel doctrines about systematic opposition, which he regarded as "natural in such a political system as ours . . . from public principle," not pique or disappointment, were slower to win general acceptance. The great trauma of Fox's political life was George III's cunning and ruthless destruction of the Fox-North ministry by exploiting their East India proposals. By what were in Fox's view unconstitutional means, the king got Fox out and Pitt in. Time and again, in contexts quite different, Fox's arguments pointed back to this searing experience. In dealing with the East India crisis, Dr. Derry does not indicate clearly enough that there were within the Company persons who favored the Fox-North proposals. Perhaps he also does not make enough of the issue of prerogative in the outcry against the ministry. Even if the king's actions were out of line, it was even more unusual to have an administration disliked by both court and country. Fox learned by experience that though the closet might be taken by storm, it could be held only by favor. The events of 1782–84 confirmed Fox in a doctrine he had always held—the supremacy of parliament as against both court and country. Yet in the regency crisis of 1788 he denied the right of parliament to fix the conditions on which the Prince of Wales should be regent and at the same time to some extent seemed to give up the doctrines of party and court influence he had professed by seeking, in an old-fashioned way, to come into office with his friends by means of the reversionary interest.

In economic matters also he showed himself to be an old-fashioned mercantilist, untouched by *The Wealth of Nations*. He opposed Pitt's Irish resolutions, Eden's French treaty, and Pitt's customs reforms, an opposition that would have shocked Cobden and Bright.

In some ways also, for all Fox's novel doctrines of party, the Whig party of 1782–94 was old-fashioned. It had a duke to lead it. It found strength in aristocratic alliances. To some extent it was given a more modern look by the financial and electoral organization created by William Adam. If Portland and Adam did the organizing, Fox and his friends provided eloquence and ideas. When, after much havering, the Portland Whigs under the impact of the French Revolution finally joined that master political angler Pitt, there were left to maintain the true Whig tradition, as Fox saw it, only Fox and his disciples. After Fox's death these men, holding together for practically a quarter of a century and through most discouraging times, came finally into their own to push through the first dose of parliamentary reform.

Dr. Derry's book is well produced, with some interesting illustrations from contemporary prints. The index, as so often happens nowadays, is barely adequate and does not include such key words as opposition and party. Instead of a bibliography there is a bibliographical note in which inevitably the most benevolently disposed will note regrettable omissions.

R. W. GREAVES
University of Kansas

WILLIAM LL. PARRY-JONES. *The Trade in Lunacy: A Study of Private Madhouses in England in the Eighteenth and Nineteenth Centuries.* (Studies in Social History.) Toronto: University of Toronto Press. 1972. Pp. xviii, 360. $15.00.

Englishmen of the early nineteenth century coped with madness principally on a business basis. The free enterprise component in institutional provision for the insane, which emerged in the eighteenth century, remained very strong well into mid-Victorian years and even beyond. But, as was the case in so many areas of social sensitivity, the freedom of the madhouse proprietor was increasingly limited by state control and restricted by competition from public institutions. The first important act of parliament to regulate private asylums was passed in 1774; it provided for licensing and inspecting madhouses both in London and in the provinces. Alas, the physicians and magistrates who were given these tasks were not up to them; and the act of 1774 proved to be the first of several more or less ineffective laws. Finally in 1845 the Lunatics Act established a "permanent, central, supervising body" to reg-

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ulate provincial private madhouses and compelled counties and large towns to provide accommodation for pauper lunatics. Although a few county asylums had been constructed in the first years of the century, most were built in the twenty years after this act. From that point on, private madhouses increasingly catered to the affluent, leaving most paupers to the public institutions.

During the heyday of private asylums (which Mr. Parry-Jones tells us was from 1775 to 1850) the society of the madhouses reflected the "two nations" of rich and poor. Most such houses made provision both for paupers ("boarded-out" at parish expense) and for the lunatic members of rich families. This internal class division was strikingly evident in advertising and was, of course, reflected in the weekly scale of charges (from about ten shillings for a pauper to four guineas for the fortunate) as well as in the residents' styles of life. At Ticehurst Asylum "several of the patients, who are persons of wealth and station, keep their own carriages and horses, with separate servants. The proprietors have also carriages for the use of the patients, and they keep a pack of beagles."

Mr. Parry-Jones reveals the trade in lunacy to have been, on the whole, an honest commerce in which the interests of the patients were generally well served. Accused by reformers of making money out of misery, the proprietors of private madhouses were suspected of incarcerating sane people, of unduly extending the stays of their guests, of maltreating patients, and of keeping them in overcrowded and unsuitable buildings. Although there was truth in these charges, it was far from the whole truth, and many of the most lurid tales were exaggerated. Mr. Parry-Jones shows that "mechanical restraint" gave way to "moral treatment" as an increasing proportion of madhouse proprietors were competent medical men and as the emphasis shifted from custody to cure of the mentally ill.

This is an important book, based on detailed research. It would have been more useful to the nonspecialist if the author had broadened his scope (even at the expense of omitting some detail about provincial private madhouses) to include the treatment of lunatics in the metropolis as well as in public asylums, prisons and workhouses, and even of those who were kept at home.

BRIAN HEENEY
Champlain College, Trent University

NORMAN BAKER. *Government and Contractors: The British Treasury and War Supplies, 1775–1783.* (University of London Historical Studies, Number 30.) [London:] University of London, Athlone Press; distrib. by Oxford University Press, New York. 1971. Pp. viii, 274. $13.00.

Professor Ian Christie of the University of London has for some time been encouraging research students to re-examine the administrative problems faced by the British government in trying to put down the American Revolution. David Syrett's illuminating study, *Shipping and the American War, 1775–1783* (1970), was the first result of Christie's efforts, and now Norman Baker's book on Treasury contracting for war supplies appears as a natural complement to Syrett. Syrett concluded that an acute shortage of sealift, compounded by the "strategy of dispersal" adopted after French intervention in 1778, would have made effective continuation of the war after 1782 physically impossible. No such striking and original conclusion emerges from Baker's investigation. Despite numerous complaints from the commanders in the field and sensational charges from the Opposition press, the government managed to feed its armies in North America until the end of the war, and strategic failure cannot be traced back to administrative breakdown in the Treasury.

The value of Baker's book is less in its argument, which tentatively and not altogether convincingly absolves the Treasury from the grosser forms of corruption and inefficiency, but rather in its painstaking reconstruction, using mainly Treasury records, of people, procedures, and problems—in short, of government at work during protracted emergency. There are chapters on the Treasury Board, on the annual process of making contracts, on the manifold difficulties of enforcement, and on the contractors themselves and their sources of supply. Administrative history is not easy to write: seldom is there an obvious narrative line, but the topical approach tends to blur the

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influence of key individuals and the sequential relationship of events. On page 43 begins a long discussion of contract negotiations in 1781 and 1782, although I still wanted a clearer picture of the earlier years. Richard Atkinson, apparently a fabulous operator with an iron in every fire, flits through the whole book, appearing as this or that part of his career becomes relevant to the discussion; but he and his partnership hardly seem typical of anything and deserve separate treatment. Syrett did a better job of describing complexity without losing his way in detail and of keeping his work firmly grounded in the context of the war itself. Less successful than he might have been in avoiding the flaws of the genre, Baker nevertheless commands our respect for the breadth of his research and his obvious control of a highly intricate subject.

But perhaps the differences between the studies of Baker and Syrett lie as much in their subjects as in the authors. Syrett's Navy Board was a large, experienced, fairly aggressive bureaucracy whose efforts to find shipping carry the historian directly into the practical work of warmaking. Baker's Treasury, on the other hand, ran its part of the war by remote control, keeping a wall of paper between itself and the real world of rotten beef and rancid flour. In any case, its chief concern was in financing the war, not feeding soldiers. The pioneering study by Stanley Pargellis, *Lord Loudoun in North America* (1933), which unravels the mysteries of provision contracting in America for the critical years 1755–57, when reread in conjunction with Baker, suggests that the Treasury in 1775 and after was trying to muddle through with old methods worked out during an earlier and simpler war when America was friendly territory. Not until the end did the Treasury find a different, more active and technically skilled set of contractors to replace the City types, chummy with the government but themselves as passive as the Treasury, who blithely passed the business down a chain of subcontractors and agents while taking a modest cut for their time and money. The best parts of the book deal with these contractors and their work, and they might have been combined and expanded because that is where the action really was, while the back and forth of Treasury routine, gleaned from the massive manuscript record, might have been compressed into a chapter.

JOHN SHY
University of Michigan

HOWARD TEMPERLEY. *British Antislavery, 1833–1870*. Columbia: University of South Carolina Press. 1972. Pp. xvii, 298. $12.95.

Having secured emancipation in their own sugar colonies, British abolitionists turned their attention to the destruction of slavery and the slave trade in other parts of the world. The enormous problems associated with abolitionism on an international scale, the varied approaches taken by the British antislavery movement to protect the liberty of West Indian freedmen and to achieve the liberation of bondsmen elsewhere, and the factionalism that arose among British abolitionists in the mid-century constitute the main themes of Dr. Temperley's outstanding book. The work tells us much that is new about the numerous antislavery societies that rose and declined in the period. Its treatment of leading personalities, their social origins and their individual motivations, is sensitive, balanced, and lively.

In the late 1830s the British antislavery movement divided on the question of force. The large, influential, Quaker-dominated British and Foreign Anti-Slavery Society, formed in 1839, committed itself to pacifism. It opposed Buxton's remedy for the slave trade as well as the Niger expedition in 1841 and the Royal Navy's use of force to apprehend slave traders on the high seas. Once asunder, the British antislavery movement was never reunited. Rather, its divisions were intensified by the bitter partisanship that arose in the abolitionist movement in America where, as Temperley argues in elaborate detail, the bulk of British attention was focused after 1850.

Dr. Temperley has worked extensively in the files of antislavery organizations and in the private papers of abolitionists on both sides of the Atlantic. The distinctions he draws between British and American abolitionism and the constitutional structures under which they functioned are highly perceptive; indeed, his analysis is always probing, poised, and interesting. If the author too often accepts at face value the observations abolitionists made on

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conditions in the colonies, this may be explained by his heavy reliance on abolitionist archives and his comparatively limited use of government documents, especially Colonial Office records. After emancipation several of the most conscientious West Indian governors, whom the planters contended were pro-freedmen, took harsh views of the proceedings of abolitionists and expressed little faith in their veracity. The author's treatment of John Gladstone's introduction of Indian laborers to Guiana in 1838 as well as Temperley's assessment of restrictions imposed on Indian immigrants in Trinidad in the mid-forties reflect the strong bias of his abolitionist sources. On the whole these criticisms are minor and do not significantly detract from a brilliantly conceived and attractively written work of considerable importance.

WILLIAM A. GREEN
Holy Cross College

D. P. O'BRIEN, editor. *The Correspondence of Lord Overstone.* In three volumes. New York: Cambridge University Press. 1971. Pp. lxxxi, 452; xxvi, 453–980; xxiii, 981–1548. $28.50 each.

O'Brien's *Overstone* puts one in mind of Piero Sraffa's *Ricardo* (1951–55). Both works offer (among other things) several volumes of correspondence documenting the development of English economic thought and policy; both disclose intriguing personalities behind often technical discussions; both are handsome productions from a great university press; and both are edited with skill and understanding and an obvious desire to assist the reader. O'Brien, for example, in his useful introduction setting out the essentials of Lord Overstone's life and thought, guides readers who may not be clear about the differences between the "currency" and the "banking" schools through that fierce mid-century controversy over English banking.

At first glance three large volumes on Overstone—whose stature as an economic thinker was scarcely on a level with Ricardo's—may seem excessive. But Overstone was a giant in the world of English banking and public finance from the 1830s until the 1860s: he was, as Schumpeter describes him, "the currency school's strong man." These volumes more than adequately justify themselves in the detailed confirmation of Overstone's place in Victorian high politics and finance. Although a Whig there is no doubt that his ideas left their mark on Peel's Bank Act. He was the friend of Lord John Russell whose admiration brought him his peerage in return for which Overstone brought to the Whig party in the House of Lords his financial expertise. He was an even closer friend of Charles Wood, Whig chancellor of the exchequer, who looked upon him as his teacher and regularly consulted him. The great friendship of Overstone's life, which furnished the "unifying thread" of his correspondence, was with G. W. Norman—a director of the Bank of England as well as a fellow banker, squire, and member of the Political Economy Club. Their letters reveal a close collaboration in urging policy and writing pamphlets.

If further justification were needed for these volumes, one might find it in what the letters offer in the way of family biography. Overstone's father, Lewis Loyd—one "l" was reputedly dropped to conceal Welsh origins—started his career as a Unitarian minister, but on marrying into the Manchester banking family of Jones he found his true calling as a banker. At his death in 1858 he left a vast fortune of something over three million pounds, about a third of which was invested in agricultural land. Overstone did even better. Born in 1796 as Samuel Jones Loyd, he grew up a devout Anglican, perfunctorily attended Eton and Cambridge, and then joined the family banking house where he remained until his peerage forced retirement from active business. At his death in 1883 he was worth about five and a quarter millions, of which about three was invested in land. It would seem that neither father nor son coveted the glories of a great landowner. Russell's offer of a peerage came as a surprise, even as something of an annoyance. The enormous buying of land—which went on into the 1870s—was pursued mainly for profit. Although an enlightened landowner, farming scientifically and building many schools, Overstone found rural politics, country sports, and countryhouse ostentation a bore. At the end, however, all of life became as ashes. He had no son; agricultural rents collapsed; and in Glad-

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stone, whom he thought to be insane, he saw the death of Whiggism.

DAVID SPRING
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DONALD N. MCCLOSKEY, editor. *Essays on a Mature Economy: Britain after 1840*. Princeton: Princeton University Press. 1971. Pp. xv, 439. $12.50.

This volume consists of the text and discussion of fifteen papers delivered at a Harvard conference in 1970, with the addition of a later contribution and an editor's introduction. The title is perhaps somewhat misleading: four of the papers deal with the origins of the "new" economic history and its prospects in British universities. The remaining papers fall into various categories. The main subject is the performance of the British economy in the late nineteenth and early twentieth centuries: examinations of the London capital market and the chemical, coal and steel, and machine-tool industries are used explicitly by the editor to question the charge that institutional and entrepreneurial weaknesses contributed to Britain's lagging performance in these years. Presumably Vamplew's abortive study of Scottish railway productivity in the period 1870 to 1900 was intended to contribute to the same debate, though its inclusion in the volume is mainly of methodological interest. Thomas's paper on American and British building cycles also has the same terminal dates. Others have different time-horizons, though two discuss issues related to the question of innovation and efficiency in late Victorian Britain—agricultural mechanization and the transition from sail to steam at sea—and Saul's later contribution reviews the question of British retardation in the light of evidence presented at the conference. Hawke's discussion of railway passenger traffic in 1865 is explicitly methodological, while Engerman's paper on relationships between the American iron industry, US iron imports, and the British iron industry between 1840 and 1860, and Moggridge's paper on British controls on capital movements in the period 1924–31 are less easy to relate to the other papers, both chronologically and in content.

However, as the editor states, it is the methodology of the studies rather than their content that is intended to supply the theme: in his view, it is the participants' use of the economist's box of tools that gives the volume a broader unity. But it is evident from the papers that these tools have been used in widely differing ways. The papers by Thomas and Moggridge, for example, are markedly different from those by David and Edelstein, and the editor's concept of this unifying methodology is not detailed in the introduction. It is not until the closing section that Hartwell's paper supplies a concise working definition of the "new" economic history as (a) the sophisticated use of theory and statistics; (b) the explicit formulation of hypotheses; and (c) the careful testing of hypotheses. Some of the papers in this volume provide valuable examples of the strengths of this particular approach to British history; some also illustrate pitfalls into which its adherents may stray.

It is the rigor of approach of the "new" economic history that is the most evident of its strengths, and this is generated by model building and the explicit formulation of relationships. But, as Hawke notes in his incisive paper, it is the logic imposed by what he terms "mathematical thinking" rather than techniques themselves that leads to closer specification. Analytical and quantitative techniques can provide powerful tools for the handling of historical questions, though, as Hawke demonstrates, the rigor of these techniques, properly used, demands an acknowledgment that some historical questions cannot be answered.

But while the use of sophisticated techniques may make economic historians more precise in their formulation of questions and structuring of hypotheses, it does not give an automatic precision to conclusions: the selection of data and choice of variables play an important part in analysis, and clearly there are dangers here, not least that the methodology may give a spurious appearance of accuracy. The value assigned to a particular variable will often depend on the researcher's judgment: as the discussion of the paper by Lindert and Trace shows, the choice of discount rate used in assessing conversion from the Leblanc to the Solvay process of soda production is crucial. In such a situation it is possible to present a range

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of possibilities, but it appears that not all exponents of the "new" economic history are prepared to make this concession to the frailty of their assumptions or data. The editor's own paper, on the British coal and steel industries, is apposite: as the discussants note, his comparison of American and British productivity depends on single-year observations, 1909 and 1907 respectively, and on the assumptions made in calculations from this data. Rigor is surely also required in the handling of data and in specifying assumptions, and this should not be overlooked in the enthusiasm for the methodology.

It would, however, be wrong to judge the "new" economic history, and this volume, solely on such grounds. Quantitative methods and the explicit use of theory provide the only means of approach to some questions: Floud's study of the productivity performance of a Leeds machine-tool firm is an example. But Floud's paper, like Lindert's and Trace's study of the chemical industry, is primarily a contribution to a synthesis, a synthesis that is far from complete, and it is perhaps symptomatic that the papers in this volume that in themselves make the strongest contributions to the historiography of the period are those in which the quantitative and theoretical analysis is only part of the argument. Papers such as David's study of the failure of British agriculture to adopt mechanized reaping techniques, Edelstein on the capital market, and Harley on the introduction of steam shipping depend as much on the use of literary evidence and the comprehension of noneconomic factors as on the less traditional analysis that contributes to the chain of argument.

On this showing, British economic historians may perhaps be forgiven for wondering why the "new" economic history has been elevated to the status of a self-conscious movement, yet in the closing section of the conference, on the future of the genre, the need for the "new" economic history to be deliberately fostered in Britain appears to have been assumed rather than demonstrated. The implications of R. C. O. Matthew's paper, which despite its shortness and restraint raised fundamental questions, and of the remarks of those discussants who attempted to relate the demands of the methodology to the realities of graduate and undergraduate teaching in British universities do not appear to have evoked sustained discussion. This is regrettable because it is surely incumbent on those who believe that British economic history requires substantial reform to demonstrate their view of the subject and its role in British culture. It is strange that a conference devoted to a demonstration of the use of rigorous techniques in economic history should not have applied the same standards of analysis in an examination of its objects.

M. C. REED
University of Glasgow

MARTHA VICINUS, editor. *Suffer and Be Still: Women in the Victorian Age*. Bloomington: Indiana University Press. 1972. Pp. xv, 239. $7.95.

Needless to say, none of the essays in this book shares the point of view of Mrs. Sarah Stickney Ellis who in 1845 advised unhappily married women to suffer and be still. Neither do the essays reflect the modestly rousing view of the editor, who begins her introduction with a reference to the women's liberation movement and closes with the reminder that "women are still largely excluded from circles of power, authority and prestige." Surprisingly there is nothing in this book about the struggle for power and emancipation. Old hats have been entirely discarded. The suffragettes do not appear, no aspect of "the cause" is rehearsed, the heroines of "the movement" are not praised—indeed they are not even mentioned. In this collection of essays politics have been abandoned, but no new coordinating focus emerges. The book, accurately reflecting the state of the woman question in Victorian historiography, has something of the tone but none of the coherence of a manifesto. Its character is more that of a magazine, and it is appropriate that five of the ten essays first appeared in the issue of *Victorian Studies* (volume 14, 1970–71) that masqueraded as *The Young Ladies Journal*.

While each of the ten essays is interesting and informative, they vary considerably in scholarly significance as well as in focus and method. On the whole those essayists who most clearly appear to be working in what the editor

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calls "the new field of women studies" make a less significant contribution than those who, remaining in more central and established fields, treat women more conventionally as subheadings. The Showalters, for example, do not successfully establish the significance of Victorian ignorance of the truth about menstruation, nor does Peter Cominos have a framework sufficiently powerful to enable him to get beyond abstract pattern making in his discussion of feminine sexuality. On the other hand, Jill Conway's impressively skillful discussion of Patrick Geddes, a surprisingly neglected figure, gains strength from being rooted firmly in the established categories of late nineteenth-century intellectual history. Jane Stedman's beautifully clear description of a minor literary convention changing under the impact of altering social attitudes is an excellent example of well-informed, traditional literary history. Helene Roberts, in a well-documented and gracefully written essay, draws on her extensive and expert knowledge of Victorian art to show authoritatively how three major themes in the woman question—marriage, redundancy, and sin—were illustrated in early Victorian painting. These three essays, along with several others in the collection, benefit from not attempting too much too soon. Without assuming the existence of a new field called women studies, these wisely cautious essayists have demonstrated how women studies might be successfully conducted.

The most generally useful and potentially productive piece in the book is Barbara Kanner's well-ordered and clearly focused bibliographical essay. Convinced that Victorian women should be studied in relation to the major themes in Victorian social history, Dr. Kanner has arranged her bibliography of primary and secondary materials to point the way toward understanding how women were affected by the most important changes in Victorian society. No one should work on the woman question in Victorian England without first consulting Dr. Kanner's bibliography. In conclusion it should be noticed that Peter Stearns's thoughtful and richly suggestive study of working-class women in the period from 1890 to 1914 reads as if it had been inspired by Dr. Kanner's convictions. This subtle and penetrating essay, enlivened by systematic comparison of the situations of English and French women, convincingly illustrates how much is to be gained from studying women in the context of established historical concerns.

R. J. HELMSTADTER
University of Toronto

DAVID TRIBE. *President Charles Bradlaugh, M.P.* Hamden, Conn.: Archon Books. 1971. Pp. 391. $11.00.

David Tribe is a talented Australian-born writer who in 1963 became president of the British National Secular Society, which was founded in 1866 by Charles Bradlaugh. This book—the first biography in over fifty years of the great freethinker and reformer—is clearly a labor of love, but no hagiography. It draws upon all the available sources of documentation. Indeed, it is the first book to benefit from a Bradlaugh family collection of papers and letters that earlier researchers had been told did not exist. The value of any such collection is undoubtedly great, though it yields no surprises.

The title of this book refers to the wishful expectation of Victorian admirers of Bradlaugh that he might one day become the first president of a Republic of Great Britain. The author has thus chosen to draw an essentially political portrait of a man who was surely as much *moraliste* as *politique*. Just the same, Tribe illuminates fascinatingly the complex rhythms of nineteenth-century republicanism and nationalism. The attacks upon "hereditary legislators," like many other issues moving Bradlaugh, were much the same as those animating the English Jacobins earlier. His program, combining such things as legal abolition of primogeniture and entail with freedom of publication, as well as his interest in congregational secular substitutes for Christian forms, all suggests something of Paine and Condorcet. Yet Bradlaugh was also a Victorian in his individualism, his Puritanism, and in the awe in which he held Parliament. Free thought was to him, as to his rival George Jacob Holyoake, the natural extension of self-help and free trade to the realm of intellect and religion.

Many other books in recent years have contributed much to the understanding of mid- and late Victorian radicalism and popular

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free thought. Those by Arthur Nethercot, Peter Fryer, A. O. J. Cockshut, Chushichi Tsuzuki, and, notably, Walter Arnstein in his judicious and thorough study of Bradlaugh's Parliamentary struggle, are particularly relevant. David Tribe's contribution is his unification of many themes separately treated by others. The narrative is swift-paced, witty, and superbly detailed. And Bradlaugh and heterodox England are all the more alive because of it.

CHARLES K. KRANTZ
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R. B. JOYCE. *Sir William MacGregor*. New York: Oxford University Press. 1971. Pp. xvi, 484. $21.25.

Sir William MacGregor (1846–1919) was successively a doctor, administrator, and governor in six British colonies—the Seychelles, Fiji, New Guinea, Lagos, Newfoundland, and Queensland. In his last post, as governor of Queensland (1909–14), he was chancellor of the University of Queensland, where Mr. Joyce works, and the focus of the biography is on MacGregor's career in the Pacific, notably in Fiji and New Guinea.

Recent biographies of colonial governors have demonstrated how difficult it is to write about so fragmented a career as that of the successful colonial governor. As the great man moves from colony to colony, each time having to come to terms with a new environment, the biography seems to come to a halt and make a new start. Instead of the biography of a single individual, what emerges is a series of monographs, held uneasily together in a chronological framework, on the problems of a string of colonies as seen from the government house.

The present biography follows the usual pattern. MacGregor was a reticent man, on uneasy terms with his family and his superiors, and one who got on with his work without bothering to set down his inmost thoughts on paper. Joyce's attempts to sketch a portrait of "the real man" are brief and fragmentary. MacGregor emerges self-made from a humble Scottish background (which is unconvincingly sketched in) and is almost at once absorbed into the official world of the colonies. There are no comparisons with other careers, no ventures into psychohistory, apart from details of the now-familiar round of complaints about distant politicians and the officials of the Colonial Office.

MacGregor's work was often interesting, and sometimes verged on the significant, but he never served in a really important colony and he never became more than a governor of the second rank. The result is, therefore, a series of learned and extended footnotes to colonial history, grouped around the skeleton of a biography. Many scholars will be grateful for the information included. But something more is needed for a biography.

H. J. HANHAM
Harvard University

ARTHUR MARSDEN. *British Diplomacy and Tunis, 1875–1902: A Case Study in Mediterranean Policy*. New York: Africana Publishing Corporation. 1971. Pp. x, 276. $12.50.

This book by Arthur Marsden is a study of a problem that, as he says, was of "hardly more than secondary importance in itself" but that "assumed a position of some significance in international affairs." It is largely a question of definition. Even before 1881 interest in the bey's domain was limited. Stanley was "rather indifferent" to its fate; Napoleon III would "not have the slightest objection if it went to Italy"; Visconti Venosta said he would "refuse it if offered on a plate." Its importance in relation to the Triple Alliance has long been discounted: few scholars would hold that it "led Italy into an alliance with Germany." After 1881 Tunis itself was a minor concern even in Rome, let alone London. What was of considerable importance to Italians was that Tripoli should not follow the same road, and, to the extent that Italy was a factor to be considered in British policy making in this period, Rome's interest in Tripoli was shared in London. Dr. Marsden never makes this distinction clear and seems obsessed with proving that Tunis itself was the prime concern. He makes a gallant attempt to give it a place in the "wider policy of safeguarding the route to India," but the fact remains that Salisbury always regarded it as part of the small change of diplomacy and gave it away for Cyprus—possibly as a prelude to a squeeze in Egypt. The author examines Salisbury's conduct in 1878 in some detail but curiously omits this hy-

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pothesis, evident from a juxtaposition of the "Egyptian" with the "Tunis" correspondence. It does not help his case that, as he shows, the Admiralty was totally indifferent even to the development of Biserta on the grounds that anything which divided the French fleet was a good thing. The only occasion on which Salisbury showed an interest in British residual rights in Tunis was in 1890 when the Manchester Chamber of Commerce forcibly reminded him of King Cotton. Even so, once the French satisfied the cotton interest he cheerfully bargained away the remainder in 1897. His attitude, in other words, was negative: Tunis was always something to be given away in exchange for French compliance elsewhere, and, since the meat was gone by 1881, diplomacy after this concerned the bones. The study is solidly based on Foreign Office, Admiralty, and private papers with occasional reference to the Quai d'Orsay. The bibliography is comprehensive, the printing at times atrocious.

C. J. LOWE
University of Alberta

MARTIN J. WIENER. *Between Two Worlds: The Political Thought of Graham Wallas.* New York: Oxford University Press. 1971. Pp. 229. $11.25.

No doubt about it! We are witnessing a great revival of interest in Graham Wallas, one of the Fabian big four, the historian of Francis Place, an indefatigable London School Board member, a father of the London School of Economics, and the author of two seminal books of political and social analysis. And though it is not quite true that "no serious study has ever been made of him" (p. vii), Wiener's is certainly the most substantial and attractive work to date, and it merits very high praise.

Wiener sees the Edwardian Wallas "between two worlds," the Victorian and our own: the Victorian elements in Wallas's outlook, especially the moral, made his generation hostile to his work, but those elements commend him to us in our time of crisis. Further, in contrast to the Webbs, Wallas's eschewal of ideology vindicates him as a prophet "of the renewed, modernized Britain sought by all parties" (p. 215). Wiener's exposition of Wallas's ideas is masterful, as is his deft placing of them within the development of social science before, during, and since Wallas's time; the author never loses sight of Wallas in mere backdrop nor makes him more original than he actually was—a temptation in intellectual biographies. Wiener finds two main springs in Wallas, the moral one of Evangelicalism (where Wiener makes imaginative use of the interpretation propounded by Noel Annan) and the intellectual spring of Benthamism. Occasionally, however, one has the uncomfortable feeling that too much is made to hinge on Evangelicalism; *noblesse oblige* doubtless reinforced Evangelical zeal. It is arguable, also, that Wallas's classical education was more essential to his outlook than Benthamism. And Wiener in effect takes this position when he insists on the primacy for Wallas of Aristotle's idea of the good life and the good society (pp. 10–11). Moreover a central theme here, Wallas's exaltation of intellect, is Hellenic. His conversion to socialism probably owed much to Aristotle.

Minor matters include Wiener's frequently confusing enclosure of single words in quotes; the absence of a bibliography of secondary works is inconvenient. Wallas's *The Art of Thought* (1926) is slighted. It is inconsonant to characterize Wallas as a "debunker" (p. 216), as "never a rigorous thinker" (p. 13); that Wallas ever viewed teaching only as a livelihood (p. 15) is, undocumented, questionable.

FREDERICK M. SCHWEITZER
Manhattan College

NEAL BLEWETT. *The Peers, the Parties and the People: The British General Elections of 1910.* [Toronto:] University of Toronto Press. 1972. Pp. xi, 548. $17.50.

That there were, in 1910, two general elections within the span of eleven months is itself testimony to the fact of an acute crisis in British political society. In a work of astonishing breadth and painstaking scholarship, Neal Blewett investigates the forces, social as well as electoral, that created such a situation. One may question some of his general observations and dispute various points of detail, but one cannot be otherwise than grateful for the wealth of material that he has assembled.

The first of the 1910 campaigns opened with a resounding speech by Asquith at the Albert

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Hall, from which women were carefully excluded to guarantee against suffragette disruption. It was a campaign of gruesome duration, which left the Liberals shorn of their magnificent 1906 majority, but easily able to continue in office with Irish and Labour support. The second campaign, more narrowly focused on the constitutional issue and as brief as the law would allow, pretty much confirmed the verdict of the first. Both occasions revealed Tariff Reform as one of the crucial, if least appreciated themes of Edwardian politics. Blewett sees them as "terminal elections" that "provide a final perspective on the politics of a generation." Others would see the watershed earlier and would argue, on the basis of much the same evidence, that the 1910 results were an anticipation of subsequent developments.

It is more in its conception than in its execution that the book is chiefly open to criticism. While some measure of background information is mandatory, the author prefaces his account with a lengthy discussion of the preceding quarter century. The second of the book's four parts, which is its core, is followed by a series of essays, loosely strung together like glass beads and varying in size and luster. It is a cumbersome structure, which makes for a good deal of repetition and does not allow the author either to sustain or justify some of his most provocative assertions. It would have been helpful had some of the material on party organization and the electoral system been incorporated into the narrative. In particular, the section on the political alignments of the churches is both too original and too fundamental to the book's thesis to be left dangling.

Despite its price, which is likely to put this volume beyond the reach of the specialist to whom it is addressed, the footnotes are buried in a common grave at the end. Given that the references include many explanatory notes and critical interjections, this is not only an inconvenience to the reader but also an injustice to the author.

STEPHEN E. KOSS
Columbia University

KENNETH O. MORGAN. *The Age of Lloyd George.* (Historical Problems: Studies and Documents, Number 12.) New York: Barnes and Noble. 1971. Pp. 233. $8.50.

There has been for some time a need for a concise account of recent research on the decline of the Liberals. Dr. K. O. Morgan has provided an admirably clear guide to the numerous books and articles that have appeared in the past four years and that have outdated most previous work on the subject.

A feature of this book is its examination of the role of regionalism in British politics. In the past the Liberals have been treated almost as if their fortunes fell at roughly the same rate everywhere. Dr. Morgan argues convincingly that Paul Thompson's study of London Liberalism deals with an atypical area, while his own book on Wales and P. F. Clarke's on Lancashire show that outside London the Liberals still had plenty of life in 1914. Dr. Morgan could perhaps have extended his analysis to consider the "nationalisation" of British politics with the emergence of a popular press with nationwide circulation—as distinct from the nineteenth century, when the local press had relatively more significance. A party, such as the Liberals, that depended heavily on individual candidates and on strong local campaigns was thus almost bound to suffer in comparison with the essentially London-oriented organizations of Labour and the Conservatives.

The Age of Lloyd George consists of a ninety-page essay on the Liberals, followed by eighty-nine documents. Although in his text Dr. Morgan shows the importance of social changes on Liberal fortunes, the documents (apart from the pre-1914 section) deal mostly with the more spectacular personal relations of the Liberal leaders.

Altogether, this is a good summary of existing knowledge, one that makes a confusing subject comprehensible to the reader, and many will find it useful.

MICHAEL KINNEAR
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E. W. R. LUMBY, editor. *Policy and Operations in the Mediterranean, 1912–14.* (Publications of the Navy Records Society, Volume 115.) [London:] the Society. 1970. Pp. xvi, 481. $12.10 postpaid.

The Mediterranean naval situation in the years immediately before the First World War was rife with difficulties for the British. Would

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the Austrians join the Italians to sweep the French from the area and would Britain with the vast bulk of her navy in the North Sea—facing the Germans—be able to hold and protect Malta, the canal, and her communications with India? The new weapons of the period—mines, torpedoes, and submarines—also complicated the problem: they gave inferior navies the capability of destroying the fleets of the major powers. The British were also wary of cooperating with the French, their ancient foes. The lack of a common signaling code and faulty liaison between the Allies led to much confusion at the outbreak of war.

While Mr. Lumby's book covers only a three-year period, it has a good selection of the detailed and definitive materials employed by researchers and archivists. Using Foreign Office, cabinet, and Admiralty papers as well as modern secondary sources, he divides the work into four parts: the debate on the roles of the various fleets before the war; the German cruisers incident; the court-martial of Rear-Admiral Troubridge; and the events in the Mediterranean up to January 1915 when Turkey had entered the war.

For at least a decade before 1914 Germany had pursued a policy of expansion in the Middle East. The Berlin to Baghdad railroad scheme and the 1907 convention between Britain and Russia, Turkey's old enemy, had strengthened German influence at Constantinople. Shortly before the outbreak of war the German battle cruiser *Goeben* and her consort, the light cruiser *Breslau,* then in the Adriatic, were ordered to Turkey. Ignorant of their destination, Admiral Sir A. B. Milne, commander in chief in the Mediterranean, was torn by his orders to protect the French ships about to bring their troops from North Africa to the Continent and his desire to eliminate the twin threat posed by the Germans.

Winston Churchill, at the age of forty, was first lord of the world's most powerful fleet and had a key part in these proceedings. His memos are usually clear and succinct, but he was hampered by the uncertainties of the first few days of the war. As a result the German ships were shadowed by British battle cruisers shortly before the war began, and when it was declared, the *Goeben* and *Breslau,* because of a dispersal of forces, were opposed by weaker ships. It was left to Rear-Admiral Troubridge to decide if the two German cruisers were a "superior force" to his four. Milne, in the western Mediterranean, never gave Troubridge a direct order to fight. At the crucial moment he turned away from battle and Milne said that "the admiral did not try." The court-martial of Troubridge for failing to engage the enemy is the most compelling part of the book. Based on earlier reports, the rear-admiral knew that the *Goeben* was faster than his ships and that her guns outranged his own. Even with his numerical superiority he believed that the *Goeben* could keep away and destroy his ships one by one. We know now that at the time of the anticipated battle, the *Goeben,* because of boiler trouble, had a speed about equal to that of the English ships, and they could have closed in on her. The court-marital board did not know this and acquitted Troubridge. But neither he nor Milne ever commanded afloat again. Churchill said of the affair that "the explanation is satisfactory, the result unsatisfactory."

After this German triumph, the fortunes of the Entente powers in the Mediterranean went speedily downhill. Turkey entered the war on the opposite side and six major ships were lost in the Gallipoli fiasco of 1915. Both German warships saw further action; the *Breslau* was sunk by mines in 1918, but the *Goeben* remained on the active list of the Turkish navy until after World War II.

While this is obviously a book for the specialist, the general reader can learn much: the day-to-day reaction of the Admiralty to the crisis, the differences of commanders on the spot, and the awful consequences of misjudgments in wartime. The maps are good, and while there is a fine index of subjects and a biographical index of persons, the work needs a glossary of terms. The meaning of "danger space," "heaters on torpedoes," and other technical terms should be explained. But the author threads his way through the thickets of verbiage and gives us a clear picture of Britain's troubled role in the Mediterranean in the years from 1912 to 1914.

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BARBARA LEWIS SOLOW. *The Land Question and the Irish Economy, 1870–1903.* (Harvard Economic Studies, Volume 139.) Cambridge, Mass.: Harvard University Press. 1971. Pp. ix, 247. $8.50.

Barbara Lewis Solow rejects the popular and scholarly consensus that defines landlordism as the root of poverty in nineteenth-century rural Ireland. She traces economic distress to a number of sources: overpopulation, inefficient cultivation, poor soil in many parts of the country, a lack of alternative occupations for tenant farmer families, subletting, and customary tenant privileges restricting landlord initiative. Solow's statistical evidence shows that tenant farmer profits in the postfamine period rose faster than rents and that evictions were rare. Then in the late 1870s potato blight, inclement weather, and the competition of American grains reproduced hard times. Exploiting peasant discontent, nationalists used landlords as scapegoats, branding them as the instruments of Irish suffering, the agents of British misgovernment. British politicians responded to the Land League with reform legislation to divert the Irish masses from the forces of extremism. Since the land acts were motivated by political considerations, Solow insists they missed the essence of the economic problem. With the increase of tenant power, landlords were reluctant to invest money or energy in a dying system and finally were happy to sell their property to tenants financed by generous government loans. Peasant proprietorship was a victory for Irish nationalism rather than a contribution to economic progress. But, as Solow concludes, "if the Irish sacrificed economic progress on the altar of Irish nationalism, who can say it was the wrong choice" (p. 204).

Solow's study is quantitative history at its best. Employing previously neglected data, she has diminished the myth of the rack-renting, evicting landlord class. And she is quite right in describing the emotions and rhetoric surrounding the Irish land question as issues of Irish nationalism and British politics rather than economics. Solow understands the core of tenant farmer hostility to landlords: they were alien, descendants of conquerors, confiscators, and planters.

Solow's study has its weaknesses. Her group statistics mask the reality of individual suffering. Landlords as a class were not brutes, but landlordism sapped the energy and ambition of the Irish people. Solow's statistical research is thorough, her presentation clear and intelligent, and her thesis original and important, but she concentrates on the economic at the expense of other aspects of Irish history. Her discussion on the politics of Irish nationalism and its relationship to the land question ignores important research published since 1963. For example, her interpretation of the New Departure rests on secondary sources superseded by Thomas N. Brown's *Irish-American Nationalism* (1966). Despite these limitations, Solow's revisionist interpretation of the Irish economy in the postfamine period should have a positive influence on the writing of Irish history.

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JOSEPH LAJUGIE, editor, with the collaboration of P. BARRÈRE *et al. Bordeaux au XX^e siècle.* (Histoire de Bordeaux, Volume 7.) Bordeaux: Fédération historique du Sud-Ouest. 1972. Pp. 746.

This volume concludes the narrative and analytical part of the *Histoire de Bordeaux* under the general editorship of Professor Charles Higounet (volume 8 will be the index). It is, like the entire work, the product chiefly of local scholars and a monument honoring a great city. Without doubt, Bordeaux and its immediate region now enjoys one of the best municipal histories of France. The work is a mine where one can find a vast array of facts; it is also a source of useful interpretations and insights about municipal life since ancient times.

Does this mean that the entire history, and particularly the volume under review, should serve as a model for others to emulate? My answer to this question is a qualified yes. The strengths of the series and of volume 7 are too obvious to be ignored: each section is an original contribution by a specialist so that the reader has at command a depth of knowledge that is rarely found in general studies written by only one or a few authors. Thirty-four scholars have written parts of the entire *His-*

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toire de Bordeaux, of which twelve, or nearly one third, combined their talents to produce *Bordeaux au XXe siècle.* Moreover, it is the most voluminous book of the series. Why then do I offer merely a qualified affirmative? My qualification results from the major weakness of the series and particularly of volume 7: the insufficiency of synthesis, of generalization. This absence is attributable not only to Professor Joseph Lajugie, the chief editor of this volume, but also to several of the contributors whose conception of history seems limited to a general narrative that often takes on the character of a catalog of names, places, and events. I am not suggesting here that names, places, and events are unimportant; but I want to emphasize that the job of the historian is to go beyond them in a search for generalization. It is a common weakness of collaborative works to emerge as heterogeneous entities made up of ill-fitting and ill-assembled parts. Each part may be excellent in itself, but is not fitted neatly to the others. Therefore, the imperative duty of the editor is to fit these parts together with such skill that their interrelation becomes obvious to the reader. The failure to carry out this task leaves the reader with the impression that municipal politics exists in a vacuum or that economic development has little connection with social structure and politics.

Counterbalancing this weakness are the strengths of each section. Professor Dupeux offers us the useful insight that during the interwar years the elite of the wholesale merchants and shipowners lost political control to the petty bourgeois masses, a loss that did not offer a threat to acquired positions because in Bordeaux socialism was but a pale reflection of its national counterpart. Indeed, according to P. Barrère, the political bent of municipal leadership seems to have had no effect on the growth and character of expanding suburbs. The affluent middle class bought up plots of land carved from aristocratic domains, developed them in an anarchic fashion, and gave little heed to the need for sewers and services. Precise urban planning was hardly a goal prior to 1939; neither was architectural beauty.

The economy also showed signs of weakness and poor planning. Joseph Lajugie ably describes the inability of the port to emerge as an international center of trade because its hinterland, the southwest, was one of the most economically backward areas of France, offering little that Bordeaux could export and a purchasing power too low to absorb massive quantities of imports. The sections on economic life are well supplied with useful statistical tables and charts. One could wish, however, that the data on trade included the value as well as the tonnage of merchandise. A ton of wine has a value different from a ton of wheat. Equally useful would be data on the volume of profits, as well as a table grouping the detailed catalog of information on various industries. The maps throughout the volume are invaluable, and the photographs a pleasant addition.

Political developments after 1944 are ably narrated by Professor J. Lagroye. It is a pity, however, that his work makes little reference to the general evolution of politics before the war in order to explain why the Gaullists captured Bordeaux and held it. One is not quite certain whether this turn represented a serious change, for Lagroye emphasizes that electoral stability has been a dominant trait both before 1939 and particularly since 1945. He also indicates that the population of the commune of Bordeaux hardly changed since 1911. It would be interesting to know whether there is a relation between electoral and demographic stability in the commune. Lagroye's explanation focuses on the tendency of voters to favor dominant men, and the stability enjoyed since the 1950s resulted from the role of J. Chaban-Delmas whose enviable versatility and political skill made him mayor of Bordeaux, a deputy of Gironde, and a minister—offices he held simultaneously for a brief while. Pluralism has rarely been a major issue in French politics. Citizens of Bordeaux favored it, reasoning that a minister, if also mayor, could use his national prestige to help his city. This reasoning proved perfectly sound.

If we are to believe Lajugie, Chaban's role was vastly important in economic development; in fact, the entire Bordeaux region has enjoyed considerable economic recovery and progress under his leadership since about 1954. At last appeared a group of technocrats capable of planning and, with new sources of finance, of stimulating expansion in every facet

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of community life. Thanks partly to the discovery of oil and gas in the southwest, but also to Chaban's initiative, the newly created region of Aquitania, with Bordeaux as its heart and head, reached its take-off and is now optimistically gaining altitude as it soars toward the year 2000. Lajugie has, as a director of regional development, played a part in this happy flight. His argument is well taken, but the more skeptical might reply that Chaban's contribution has been exaggerated, for the whole economy of France took off about 1954; the Gaullists in Bordeaux, therefore, profited from a national phenomenon for which they can take no credit.

Book 3, dealing with religious practice, education, art, literature, sports, and daily life, has the strengths and weaknesses of the rest of the volume. All in all the reader comes away with a multifaceted view before him, perhaps somewhat too abstract. One sees the city, one senses the ups and downs of the port, one knows that there are administrators and businessmen and that the conditions of life have changed over the century. But the human visage rarely emerges from this panorama of new skyscrapers and classic façades, of ships and loading cranes, of oil tanks, refineries, and wine casks. The book has caught everything—except the soul of the city.

LEO LOUBERE
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ANDRÉ KASPI. *La mission de Jean Monnet à Alger, mars–octobre 1943*. (Publications de la Sorbonne, Université de Paris—Panthéon-Sorbonne. Série Internationale 2.) [Paris:] Éditions Richelieu. 1971. Pp. 240.

The diplomatic history of North Africa since 1941 is especially significant; in it is found the key to many of the Franco-American difficulties in the postwar period as well as important insights into the development of the independent states of what was French North Africa. Jean Monnet played a crucial role, little understood because he wrote no memoirs and because his private papers have been and still are inaccessible to the public. The author, who has had access to these papers, has made an important contribution to the body of information hitherto available.

The United States was drawn unwillingly into the maelstrom of North African politics on November 8, 1942, in support, primarily, of British strategy. The decision for the American landing in North Africa was made by President Roosevelt over the objections of his Joint Chiefs of Staff. Intelligence estimates for the landings and subsequent policy decisions were based upon fragmentary—and erroneous—information, provided for the most part by Robert D. Murphy and his staff.

Murphy, so Walter Lippmann charged, was a naive individual who took his political coloration from his associates, who, after the fall of France in June 1940, were the functionaries and sympathizers of the Vichy government. Resolutely hostile to General de Gaulle, Murphy gravely underestimated the strength of Gaullism in French North Africa and attempted to create a third force, untainted by the collaboration charged against the Vichyites, but also untainted by the "dissidence" of General de Gaulle. Upon the recommendation of Jacques Lemaigre-Dubreuil, General Henri Giraud, a man whom neither Murphy, his staff, nor President Roosevelt had ever met, was chosen to lead the third force. Giraud, inept politically, was unacceptable to French military forces of both Vichy and de Gaulle persuasion, but he continued to be supported by the United States.

Monnet sought to effect a compromise between de Gaulle and Giraud by creating a French Committee of National Liberation with the two generals as copresidents and in which personalities would be submerged in a "collective power." His mission was successful, although Murphy charges that Monnet "betrayed" Giraud and passed "bag and baggage" into the de Gaulle camp.

The greatest shortcoming of the book is in perspective, a fault the author acknowledges "with the regret of the alpinist who must stop before reaching the summit seen from afar." As a foreigner he had only limited access to American archival sources, and he has placed too much emphasis on published American accounts while ignoring French studies such as those of Réne Richard and Alain Sérigny,

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Jacques Soustelle, Gabriel Esquier, Jean Chrétien, Alain Darlan, and others that illumine this diplomatic tangle. The positive contributions more than offset the shortcomings.

LEON B. BLAIR
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IRIS M. ZAVALA. *Masones, comuneros y carbonarios.* (Historia y arqueología.) [Madrid:] Siglo Veintiuno de España Editores. 1971. Pp. 363.

The political activities of Spanish secret societies have been the inspiration for a good deal of impassioned polemics, and an up-to-date and impartial account has long been needed. Professor Zavala proposes to examine these societies between 1800 and 1850 in order to discover their impact on Spanish political parties and their possible connection with other conspiratorial groups. Moreover, following Lefebvre or Rudé, she seeks to discover through the secret societies the role of the people in the political turmoil of those years.

About half the book is devoted to the author's text; the balance is a long appendix of documents. The approach is basically narrative and pivots on the shifts in the political regime between 1800 and 1850. The core of source material is drawn from Inquisition papers and the secret archives of Ferdinand VII, but biographies, memoirs, and numerous other sources are used as well.

Zavala finds the focus of the Spanish Revolution shifting progressively to the left. Commencing with the elitist liberal groups centering around Masonic lodges, by 1823 the movement had come to have republican elements and to reflect the grievances and ambitions of at least certain sectors of the people. Twenty years later the revolution had begun to draw sustenance from the new class of industrial workers. The author sees the conspirators as playing a vital part in establishing democratic political parties and sentiment in Spain. Moreover, she succeeds in establishing a strong presumptive case for the connections between Spanish and European revolutionaries. In this, as in other instances, the Pyrenees were not quite so high as we have sometimes thought.

Zavala succeeds in carrying through her intent. In addition she gets us at least partly inside that peculiar revolutionary world of ambition, enthusiasm, and betrayal and gives us an insight into the personalities and ideas of liberal Spain as they emerged from conspiracy into open activity. However, I do not think that she quite succeeds in "going to the people." The nonliterary, nonpolitical conspirator remains shadowy. He is there, but his role, his motivations, and his impact are blurred.

In any case, this clarification of the influence of the secret societies and their political role is well done, and the author is to be congratulated on a worthwhile book.

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DOLORES IBÁRRURI *et al.*, editors. *Guerra y revolución en España, 1936–1939.* Volume 3. Moscow: Editorial Progreso. 1971. Pp. 277.

The present volume of the Spanish Communist party history of the Civil War covers the period roughly from the fall of Málaga through the failure of the offensive against Belchite (February–October, 1937). The most important events dealt with are the war in the Basque provinces, the Battle of Brunete, the problem of Mediterranean "piracy," and the internal struggles with the Caballero Socialists and the anarchists. This is "official" history, and its value for scholars can perhaps best be illustrated by reviewing the coverage of a single famous occurrence: the political crisis in Barcelona during the first week of May. The outline of events—attempted government occupation of the telephone building on May 2, barricades and street fighting until May 5, divided counsels among the CNT and POUM forces, and an estimated five hundred killed and one thousand wounded as the balance for three days of fighting—reads in this version much as it does in the accounts of non-Communist historians. However, the FAI and the POUM are alleged to have been "better armed" than the government forces and to have paralyzed the city through a "perfectly orchestrated" plot to assault all government offices, and Ambassador Faupel's memorandum reporting that Franco claimed that his agents in Barcelona had caused the street fighting is taken as incontrovertible evidence that the

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"putschists" were in league with the "fascist" enemy. The account mentions that the anarchist ministers Federica Montseny and García Oliver came to Barcelona, but it does not credit them with a major role in calming spirits and limiting the bloodshed. There is indignant reference to the assassinated UGT officials but no mention of the several anarchists assassinated. Largo Caballero is held responsible for the crisis, but more in sorrow than in anger. There are copious, and familiar, quotations from the contemporary press and from published memoirs. Thus three decades after the events the Communists stick very closely to their 1937 version but are less polemical in their treatment of non-Communist leaders such as Largo Caballero, Azaña, and Luis Companys. Scattered throughout the text are quotations from Communist party archives, but these do not contribute important new facts to our knowledge. The volume will be most useful to students of party history, of the internal political struggles of the Left, and of the poignant half-truths of bourgeois-proletarian cooperation in 1937.

GABRIEL JACKSON
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HORST LADEMACHER. *Die belgische Neutralität als Problem der europäischen Politik, 1830–1914*. Bonn: Ludwig Röhrscheid Verlag. 1971. Pp. 536. DM 68.

DAVID OWEN KIEFT. *Belgium's Return to Neutrality: An Essay in the Frustrations of Small Power Diplomacy*. New York: Oxford University Press. 1972. Pp. xii, 201. $13.75.

No individual small nation-state in the modern period has been more significant and critical in the international politics of Europe than has tiny Belgium. The specific means that allowed Belgium to remain an independent and sovereign state was the nonaligned status called neutrality. We now have two major achievements on the theory and practice of that delicate and intricate statecraft. This long-isolated and neglected area is now illuminated by the lengthy, detailed, and valuable Lademacher monograph and the short yet precise and interesting Kieft account. While each deals with Belgian foreign policy, the former stresses the broader, fuller European context of the nineteenth-century great powers system while the latter dwells more exclusively on domestic factors as the vital determinants in the interwar diplomacy of Brussels. These are studies for specialists, whether the interest is in Belgium, neutrality, alliance systems, the sources of international conflict, and so on.

The fresh, comprehensive, very German view of Lademacher covers the years of the "real" neutrality from its inception at London between 1830 and 1839 down to its violation in 1914. The thoroughness of the research is unequaled among the similar accounts previously published. In the same manner as the multivolume Bonjour treatment of the Swiss case this rich study contributes a wealth of detail, balanced judgments, and a sound understanding of the historical forces. While the basic findings will not startle any well-informed student or ignite any far-ranging controversy, the immense industry of Lademacher results in a most welcome complete review and assessment of those eighty-odd years. The author declares and documents fully the initial success of perpetual neutrality as a "concensus status" chosen and guaranteed by the major powers functioning in a relative equilibrium. In what amounts to a historical version of systems analysis, Lademacher depicts this early phase juxtaposed to the later century with its decline in the viability of neutrality. This he attributes to the waning of the natural self-righting balance (as exercised by Great Britain) and to the disintegration of the almost institutionalized conference system. Thus the fifties and sixties are a primary concern and receive as much emphasis as the later years with the clear-cut power-bloc system attached to quasi-permanent military and friendship alliances. Lademacher demonstrates how the documents spell out the Belgian (and British) ability to cope deftly with the required rights and duties of nonbelligerents in war and of noncommitted or nonaligned in peace. It is in the exact substance he gives to generally known hypotheses that the author catches the historian's attention. For example, he does not merely assert but produces diverse sources illustrating why the relatively equal major states accepted the neutrali-

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zation of lesser states, attempting to eliminate strategically and economically relevant areas like Belgium (and Switzerland in 1815 and Luxembourg in 1867) from the European competition. The relative equality in power that characterized the balanced system, Lademacher believes, was gradually eroded by the rapid development of German power, so strong by 1900 that it no longer had to accept the constraints of the old system. Belgium therefore became a primary issue among the powers after Crimea in terms of the emerging "bipolar patterns of international behavior." One reaches the conclusion that the turn-of-the-century "bipolarity" made neutral status for a geopolitical unit like Belgium completely untenable.

Lademacher's greatest strength is his ability to focus in on the new European conditions in terms of what caused them and what they caused in the basic alteration of the power structure. Here for the first time the imperialist designs of Leopold II and especially the Congo venture are viewed as involvements that clearly forced Brussels into the competitor role. The period and issues of imperialism consume a healthy part of the volume, but the questions of military arms, alliances, and defense policy get more attention when Belgo-German and Franco-German relations become the nexus of European politics after 1905. The middle pages (155–379) are by far the best on these topics in print today. Lademacher promises a follow-up volume on the post-1914 era.

This book is marred by small flaws in spelling (in English and French titles, citations, and words) and the author's dry, occasionally slow-moving narrative. Yet patience is amply rewarded, for not only is the extensive scholarly apparatus of a high quality, but the entire elaborate development and careful interpretation of the evidence makes this work a fine example of what sound and energetic research can accomplish.

The companion essay by Kieft is more limited to Belgian interwar policy and the final decision to adopt a "policy of independence." This was not neutrality in the traditional sense for it did not center on the legal problems of nonbelligerents (yet the author misses the significance of the Article XVI debate), it did not reflect a true impartiality, and it was requested by the small state and not imposed by great powers. Kieft's original doctoral dissertation did not emphasize as strongly as the published work the interplay of domestic and foreign policy elements and especially the ultimate influence of certain demographic, economic, and political problems on the transformation of Belgian diplomacy. The compressed book makes the need for internal harmony and unity in the mid-thirties the major factor behind the October 1936 statement of the king. Although much of the basic evidence for this thesis has appeared before, the author fittingly presents the rise of dual extremism (both Flemish and Walloon) and its disruptive consequences in depression-riddled Belgium. It might be noted that not enough social structure, demographic and linguistic composition, and plain geography are presented directly in these pages, which analyze the binational Belgian community, for Kieft's conclusions to be comprehensible to anyone except a specialist well versed in very contemporary studies. One is grateful though for this intelligent synthesis and its theme of the linkage of internal and foreign affairs, although more extensive scouring of the literature, both original and secondary, could have buttressed the major assertions. Beyond more explorations of the Rexist and Vlaamsch Nationaal Verbond writings, the Van Langenhove Rhineland article ("La Crise fatidique du 7 mars 1936," *Bulletin de la Classe . . .*, 53 [1967]: 134–63), and Robert Rothstein's observations in *Alliances and Small Powers* (1968), one might query the lack of oral history evidence. The omission of the interview is important since many policymakers of the thirties were still alive when this research was in progress.

The book becomes much more controversial, however, when it proceeds to its other major thesis on the nature of the big power connections in the thirties. Interestingly and perhaps correctly, Kieft accepts Van Zeeland's argument that Great Britain and not France was Belgium's foremost and true ally. Although this recasting of the Brussels-Paris-London relationships results in a provocative and insightful series of perceptions, it also necessitates playing down the impact of certain Franco-Belgian ties that obviously conditioned the new policy of

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1936 (for example, the Franco-Soviet pact and its alienation of the Belgian bourgeoisie with its fear of being drawn into a Bolshevik war). If Kieft is correct and the British were central to Belgian policy formation, then one is struck by the fact that British thinking was based on a "warped vision" that was transmitted to Belgium and made for distortion there, too. This approach also fails to explain forcefully the prolonged effort and retarded recognition by Brussels of the unwillingness of Paris to provide the necessary formal counterweight to Nazi Berlin. Maybe Alexis Saint-Leger and not Van Zeeland was more to the point when Saint-Leger castigated the Belgians for sacrificing their real interests in order to please London when, in fact, a growing identity of interest between the Continental neighbors was emerging as the threat of war increased. Kieft is, like many of the recent generation, very sympathetic to the policy reorientation primarily because of the domestic motivations, but his analysis is incomplete for it does not explain fully the failure to reconstruct the old Western entente or security pact. He explains well the whys but deals less skillfully in his exposition of the several admittedly bad alternatives that Belgium had in 1936. Kieft argues that the Belgians were sagacious in ending the accords of 1920, since they achieved military reform and expansion and domestic tranquility and unity. Not only is it possible that the former was meaningless and the latter quite temporary, but this method of procedure omits the high prices that were forced on Belgium when she chose "neutrality."

Whereas Lademacher is coolly clinical in his analysis and basically convincing, Kieft is sometimes hurried, less than complete in his range, and basically more persuasive in his "domestic crisis" portions. But on balance the American's work is a useful addition to the cumulative scholarship that is both reshaping our views of that period in diplomatic history and clarifying the bases of small power behavior in world politics. Given the paucity of published monographs on the subject where most worthwhile literature is in article form, the Kieft effort is a needed and impressive one.

Finally, no comment is made but attention is called to the price of the 188-page Oxford production, which is *not* the result of plates, illustrations, maps, or any other expensive trappings.

PIERRE-HENRI LAURENT
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A. M. VAN DER WOUDE. *Het Noorderkwartier: Een regionaal historisch onderzoek in de demografische en economische geschiedenis van westelijk Nederland van de late middeleeuwen tot het begin van de negentiende eeuw* [The Noorderkwartier: A Study in the Demographic and Economic History of Western Parts of the Netherlands from the End of the Middle Ages till the Beginning of the Nineteenth Century]. In three volumes. (A. A. G. Bijdragen, Number 16.) Wageningen: Afdeling Agrarische Geschiedenis, Landbouwhogeschool. 1972. Pp. 258; 259–614; 616–858.

After the appearance in 1957 of the insufficiently well-known study of the rural economy of the Dutch province of Overijssel by B. H. Slicher van Bath, *Een samenleving onder spanning*, work was begun on producing similar studies of other Dutch regions. As a fruit of that labor a massive three-volume work has now appeared by A. M. van der Woude. *Het Noorderkwartier* is a study of the demographic and economic history from the sixteenth to the early nineteenth century of a region immediately north of Amsterdam.

This region of some forty-odd villages and towns is of enormous importance to an understanding of Holland's economy. Here were located the grandest of the seventeenth-century land reclamation projects as well as seafaring, industrial, and agricultural activities of international importance. It is a strength of the book that the author has consistently striven to combine a meticulous concern for village-level developments with an awareness of the national and European-wide significance of those developments. In its exhaustiveness and its systematic approach *Het Noorderkwartier* can be compared to the works of the leading French "Annales school" historians.

The first volume of this work is primarily a study of the demographic characteristics and trends of the region. The widespread mobility and the religious diversity of the region make a sophisticated use of parish registers impossible. Van der Woude had to content himself, there-

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fore, with a painstaking study of every imaginable sort of statistical material from which population estimates could be derived. He often shows ingenuity in this labor and the population trends are convincingly estimated. The basic trends are these: an explosive rate of growth in the sixteenth century brings the population to a peak around 1650; then, a continuous and catastrophic contraction ensues that lasts nearly a century and represents one of the greatest losses of population recorded in early modern European history. Van der Woude can demonstrate that neither emigration nor mortality crises caused by war, famine, or plague can account for this collapse. In contrast to many French studies, periodic Malthusian crises play no role in Holland. Van der Woude's analysis of the vital rates that stand behind this catastrophe suffers from the weakness of the parish registers. One can quarrel, therefore, with his hypotheses about the demographic mechanism that brings about the observed trends.

The basic cause of the demographic collapse is convincingly described in the second volume. Here van der Woude synthesizes the secondary literature and his own research to describe systematically the secular trends of every significant branch of economic activity in this uniquely varied region. He offers studies of seafaring, fishing, whaling, shipbuilding, lumber milling, canvas weaving, paper making, oil pressing, bleaching, as well as agriculture, and concludes that "chronic malaise in almost every sector of economic life let a flourishing region sink away into poverty and insignificance" (p. 102). The author is convinced that the population trends were not an exogenous force but were dependent upon the economic fortunes of the region.

The book, while certainly definitive in its demographic analysis, does not pretend to say the final word in its economic analysis. (In fact, van der Woude performs a service in specifying subjects for further research and indicating the available sources.) The nature of the "chronic malaise" is still not completely understood, and more research will be needed before we can be certain of the author's belief that the decline of the Noorderkwartier, where the demographic trends were more severe than elsewhere in Holland, simply throws into sharper relief the trends that were common to the entire maritime region of the Netherlands.

For a nation whose international connections have often attracted interest, we now have an excellent study of the domestic economy of a key region and a major contribution to the much-discussed problem of the decline of the Dutch economy.

JAN DE VRIES
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H. A. ENNO VAN GELDER. *Getemperde vrijheid: Een verhandeling over de verhouding van Kerk en Staat in de Republiek der Verenigde Nederlanden en de vrijheid van meningsuiting in zake godsdienst, drukpers en onderwijs, gedurende de 17e eeuw* [Temperate Freedom: An Essay on the Relationship of Church and State in the Republic of the United Netherlands and on Freedom of Expression in Religion, Press, and Education, during the 17th Century]. (Historische Studies, uitgegeven vanwege het Instituut voor Geschiedenis der Rijksuniversiteit te Utrecht, 26.) Groningen: Wolters-Noordhoff. 1972. Pp. 302. 32.50 gls.

It is a commonplace of historical knowledge that the Dutch Republic was par excellence the haven of liberty in Old Regime Europe. Enno van Gelder, one of the most able and productive of modern Dutch historians, subjects this assumption to systematic scrutiny in this study. The conclusion he reaches is crystallized in the title itself, "tempered freedom." Dutch liberty was indeed "moderate" or "temperate," the sense clearly intended by the author, because it was incomplete, sometimes uncertain, the product of both greed and principle. Yet it was "tempered," too, in another sense, made stronger because it had to be maintained against opposition, primarily from the orthodox Calvinists who sought to make the republic a greater Geneva. This is hardly a new picture, and it has been studied many times in detail. It is, in fact, the voluminous published source materials and the monographic literature that enable Enno van Gelder to explore the subject effectively without engaging in the archival investigations on which his earlier volume, *Vrijheid en onvrijheid in de Republiek*, rests. It is, therefore, not a book with new discoveries or an original vision. Its utility lies rather in its systematic overview and in its clear, temperate exposition, especially in the

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complicated question of the place of the Reformed Church as the not-quite-established church in a not-quite-Calvinist state. The final chapter, which examines the debate over freedom of opinion in the United Republic, is less than satisfactory, perhaps because the author, to eke out the paucity of Dutch theorists of freedom of thought and expression, gives us all too elementary summaries of the ideas of Milton, Locke, and Bayle.

HERBERT H. ROWEN
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KERSTEN KRÜGER. *Die Einnahmen und Ausgaben der dänischen Rentmeister 1588–1628: Ein Beitrag zur frühneuzeitlichen Finanzgeschichte.* Marburg: N. G. Elwert. 1970. Pp. 329.

POUL ENEMARK. *Studier i toldregnskabsmateriale i begyndelsen af 16. Århundrede: Med saerligt henblik på dansk okseeksport* [Studies in the Documents of the Customs Accounts at the Beginning of the 16th Century: With Special Attention to the Danish Cattle Exports]. In two volumes. Århus: [Århus Universitet.] 1971. Pp. 349; 303, 6 plates.

These two books have much in common in addition to their obvious concern with aspects of economic activity in early modern (sixteenth- and seventeenth-century) Denmark. Both are intensive explorations in sometimes very difficult source material stemming from the fiscal interests of the Danish Crown, and in both cases the authors seem more heavily concerned with the materials and a correct methodology of exploitation than they are with any particular conclusions (although, almost in passing, they also present many of the latter). Both authors explicitly leave many questions raised but unanswered. Both provide innumerable items of interesting and well-documented information. Yet the works are very different.

The Krüger book, a dissertation that investigates and attempts to summarize the income and expenditures recorded by the "rentekammer" in Copenhagen from the beginning of Christian IV's reign to the end of his disastrous intervention in the Thirty Years' War, presents, manipulates, and explains figures (probably one-third of the work is in table form). Krüger, who repeatedly warns that the "rentekammer" records do not show all the monies passing through state hands, provides a very useful overview of the finances of the realm during forty years of great activity and change. An effort is made to trace and explain the rise and fall of different types of revenue and to indicate their relative importance. Expenditures are given less intensive treatment, but sufficient examples are presented at least to provoke the reader's curiosity about the fascinating ebb and flow of political and economic life outside the countinghouse door. Krüger does not take us through but focuses resolutely upon the figures, the organizational and bookkeeping mechanics of the time, and the problems of extracting correct and meaningful information.

Poul Enemark works intensively with a much smaller and more fragmentary body of source material—toll records from Denmark (and Holstein) dating from the early sixteenth century. He, too, leaves questions raised and unanswered but only because the answers are not found in or about his source material. Indeed, his two volumes pretty well exhaust his material. He milks it of every conceivable sort of information, and the hard statistics on trade and revenues that emerge are clearly subordinate to the detailed, lovingly presented picture of times and people. Although the book is advertised as looking especially at the export (southward) of cattle from Denmark, it details all that is known of the total trade passing through the control points in towns such as Gottorp, Ribe, Alborg, Kolding, etc., and not only cattle and horses, but merchants' wares of all sorts (subject to land transport) were involved. Enemark improves upon much earlier work to present a detailed description of the toll system, the merchants and drovers, the monies, the packaging, the fairs, the laws, and so on. He winds up with an exhaustive investigation into the names of the persons engaged in the traffic, attempting to clearly identify everyone in spite of varied spellings and usages and to establish which ones were Danes or Germans or otherwise, and then he tracks them through their movements. The book will be valued by genealogists and philologists. It will become a classic in the field of early modern economic history. The German summary is good enough to tempt people to try the Danish.

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STEWART E. FRASER, editor. *Ludvig Holberg's Memoirs: An Eighteenth Century Danish Contribution to International Understanding.* Leiden: E. J. Brill. 1970. Pp. xviii, 289; portfolio of 33 engravings.

The international reputation Holberg gained in his day remains extant among students of the Northern literatures. As phrased here (p. 9–10) he "was, with the exception of Voltaire, the most prominent European writer of his generation" (for similar recent estimates see the James McNelis edition of *Niels Klim* [1960] and the article "Holberg" in the *Britannica*, 1972 printing). However, intellectual history in general has for a century been losing sight of him. This new edition of the memoirs, in English, is a reminder that he may, in some circles, merit fresh attention.

Holberg was born in Bergen but made and spent his career in Copenhagen. A two-fold objective moved him—to create the basis for a secular, urban culture within the Dano-Norwegian heritage, one to make it worthy of admittance to the wider republic of European letters and, in reverse, to bring the best of those letters to his own countrymen. His writing effort proved prodigious and diverse—in drama, satire, natural law, moral philosophy, topography, and history. His work in the latter was substantial; he wrote at length on Europe's general history, the Church, the Jews, and his own polity (on Denmark, in three long volumes). In the custom of the day, he drew heavily on recognized authorities, classical as well as contemporary, being well informed on European post-Reformation historiography. Yet he extended himself to get at documentary and independent sources. He had clear and precise ideas on the premises and procedures that should govern his historical work; on these he elaborated from time to time (see p. 235–36).

The problem of nomenclature has troubled Danish editors (cf. A. Kragelund, *Ludvig Holbergs tre Levnedsbreve* [1965], 1:vii–xii), but there seems no better word in English for these recollections than "memoirs." Various sections dwell on the personal background that only an author can provide to explain the rationale of his own works (Holberg had his share of literary feuding). Many travel experiences, some humorous, others irritating, are detailed; the accounts are derived from five trips abroad (no small distances covered on foot) and reflect contacts with common folk no less than with persons of prominence. Book lovers will find interesting his experiences in prominent libraries, particularly in Oxford, Paris, and Rome.

The "memoirs" were written as three successive "Letters" and, as such, published in 1728, 1737, and 1743. They appeared in Latin but soon were translated, and often reprinted, in continental languages. However, the English reader has long been dependent on a tightly printed booklet (London, 1827), not always easy to come by. There are no essential differences in the wording of the text between the old and new English printings, but as a publishing venture the 1972 version is much more than a reissue. It includes a fourth "Letter"—in fact a separate essay from another work of Holberg's—and the text is separated into appropriate sections with titles for divisions and subdivisions. The new printing is heavily footnoted (recent Danish editions also lean to this practice); most of the entries summarize careers of persons mentioned in the text. In the main these are very helpful, but the type of reader who is likely to consult Holberg will find some of the notes (on Homer, Cicero, Calvin, Grotius, for example) too obvious and more distracting than if they were limited to brief identifying phrases. The index covers proper names well, but concrete subjects and abstract ideas have been slighted.

This Fraser edition is handsomely embellished. There are thirty full-page prints, chiefly portraits, many from originals in the historical print collection of the Royal Library in Copenhagen. Separately packaged is a portfolio of thirty-three engraving illustrations—city prospects, public buildings, etc., from Holberg's day. About ten lack clear indication of provenance. The reader may find the detached format a bar to effective use in the absence of better instructions, as might have been included with the listing on page xi, on the relationship of each print to the text. Varying size (the larger ones are in the twenty-two- by twenty-four-inch range) also inhibits prompt use. But the facilities of print collections will accommodate them well.

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A bright new edition of the Holberg "memoirs" is most welcome. Members of our guild whose first concern is eighteenth-century cultural history in general should not overlook it.

OSCAR J. FALNES
New York, New York

PEKKA KALEVI HÄMÄLÄINEN. *Nationalitetskampen och språkstriden i Finland, 1917–1939* [Nationality Struggle and Language Strife in Finland, 1917–1939]. Helsingfors: Holger Schildts Förlag. 1969. Pp. 274. 35M.

Finland had been an integral part of Sweden for nearly six centuries and an autonomous grand duchy within the Russian empire for more than one century when it achieved independence in 1917. Throughout that period its administrative and cultural language was Swedish even though the vast majority of the population spoke Finnish. The struggle for the recognition of Finnish emerged in the train of nineteenth-century nationalism, but not until after independence did the struggle between the two language groups explode with full force, and it ran its violent course during the interwar period.

The struggle was essentially political. The new democratic principles of society and government threatened old class privileges, which were symbolized by the Swedish language. The Swedish-speaking elite dug in and aggressively sought to save their traditional position. Even theories of racial superiority were proffered in justification of their privileges. A young new breed of Finnish intellectuals struck back under the banner of "pure Finnish" (prominent among them was a young agrarian politician by the name of Urho Kekkonen). They infiltrated the political parties and put through their program. When the struggle was called off at the approach of World War II they had won. But Finland remained a bilingual country by virtue of constitutional guarantees.

Professor Hämäläinen, an American historian of Finnish background, has tackled this controversy with great skill and sensitivity, and one must hope that the original English-language version will soon be published also. His study is based on an exhaustive examination of primary and secondary Finnish sources. As a clear description and mature analysis of a largely unique problem in nationalism, it deserves wide attention.

H. PETER KROSBY
State University of New York, Albany

CHARLES E. MCCLELLAND. *The German Historians and England: A Study in Nineteenth-Century Views*. New York: Cambridge University Press. 1971. Pp. ix, 300. $16.50.

Professor McClelland's revision of his doctoral thesis is a valuable and impressive contribution to the study of German history. Few present-day historians have been through as many German histories of England; and in addition McClelland has combed the archives for information on the personal views of the authors of these histories. The book is very well organized and written with economy. There is a good index, a biographical section, and a wide-ranging chapter of critical bibliography.

From the eighteenth century on, England represented to German intellectuals—in McClelland's words—"a complex of values, associations, aspirations, and feelings." Before the French Revolution, what men like Justus Möser and Johannes von Müller admired in England was her "freedom"; publication of Burke's *Reflections* in 1791 made him almost the "ideal Englishman" for his condemnation of revolution and praise of tradition and "organic" change; the wars to 1815 exhibited England to Germans as keeper of the balance or champion of European liberties, or both; for much of the nineteenth century she was admired by liberals for her parliamentary government and by conservatives for her governing aristocracy. "The English flag flew alongside virtually every political banner in nineteenth-century Germany at one time or another" (p. 233). The shift from admiration (up to 1870) to indifference at first and then criticism, suspicion, and finally hatred, is carefully traced: coolness began with unification and German economic growth, while tempers became heated in colonial and naval rivalry.

McClelland suggests (p. 163) that the most vocal nationalists and imperialists—Treitschke, for example—worried most about the instabil-

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ity of the "new Germany," and so they engaged in a "last-ditch effort to unite non-socialist Germany in a common front in defense of a political system in which they only half believed." Though this thought is advanced tentatively, it is perhaps the book's most provocative concept. The theory that antiforeign sentiment was necessary to cement together a crumbling empire could, of course, be offered as the shoddiest of excuses by a propagandist, but it is clear that the author has no such intention. One good job deserves another, and I would like to see Professor McClelland study this particular aspect while the wealth of material he has gathered is still at hand. Was not the startling growth of socialism counterbalanced by declining emigration and rising trade?

DAVID I. GAINES
City College,
City University of New York

ROBERT M. BIGLER. *The Politics of German Protestantism: The Rise of the Protestant Church Elite in Prussia, 1815–1848.* Berkeley and Los Angeles: University of California Press. 1972. Pp. xiv, 300. $15.00.

Church and politics have been interwoven strands in German history in each succeeding century. The particular legacy of Luther's teachings on church and state has been challenged but not broken in times of political turbulence and quickly reaffirmed in times of political reaction. Dr. Bigler's study of the post-Napoleonic era in Prussia illustrates this process of restoration by which the royal authority of Frederick William III and IV was used to tighten the political control over the activities of the clergy and to impose an ideological centralization that was to prove stifling to all efforts at liberalization and reform. The failure of the church to support the radical movement in 1848, the author contends, can be traced to the successful pressures of the right-wing elite among the Prussian clergy, which represented one of the most determined, ideological, and politically united factions defending the traditional political and social order. Bigler's careful analysis of the lives and thoughts of the leading Prussian Protestants provides a useful counterpart to William Shanahan's study of the church's attitude to social problems. Bigler's emphasis on the political impact of the theology professors and the Pietist aristocrats is rightly placed, and he demonstrates convincingly how the inward-looking Pietism of the eighteenth century took on deliberately political overtones in reaction to the godless atheistic attacks of the French Revolution. The combination of royal authority, social preservation, and antirational theologizing molded the Protestant church in Prussia for the remainder of the century.

Nevertheless, one must ask whether Dr. Bigler has his perspective correctly adjusted. His predilection for the radical theologians leads him to one-sided criticisms of the repressive authoritarianism of the church hierarchy. And his final sentence, in which he claims that the failure of the German Protestants to side with the people in 1848 alienated the masses and drove them to embrace the secular religions of Marxism and National Socialism, is surely far-fetched. This voluntarist approach is too North American. A comparison with England would show that, even with the assistance of liberal political forces, the radical theologians were incapable of countering the tide of Evangelical piety or Tractarian traditionalism. German Protestant radicals were in fact as dogmatic, authoritarian, and unsympathetic to popular needs as their opponents. There was an atmosphere of intolerant factionalism in the theological infighting that escapes Bigler's notice. The radicals' readiness to indulge in iconoclasm and personal abuse was more destructive of the church's mission than he allows. The reaction of the traditionalists was prompted not only by the desire to preserve the social privileges of the Prussian elite, but also by the need to prevent the skeptical undermining of the church's teachings or the heretical identification of political radicalism with the cause of Christ. As was to be the case again in the 1930s, the legacy of Luther's vehement attacks on the radical reformation provided explosive ammunition in the theological controversies of the early nineteenth century.

JOHN S. CONWAY
University of British Columbia

DAN P. SILVERMAN. *Reluctant Union: Alsace-Lorraine and Imperial Germany, 1871–1918.* University Park: Pennsylvania State University Press. 1972. Pp. 262. $11.50.

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In this monograph Professor Silverman follows the lead of historians like Theodor Schieder and Hans-Ulrich Wehler by using the history of Alsace-Lorraine to illuminate Imperial Germany's domestic problems. Silverman examines both political policy formulated in Berlin and the evolution of political life in the provinces. The result of his research strengthens our impression that the *Reichsland Elsass-Lothringen* was both symptom and source of important structural weaknesses within the Reich itself.

As was so often the case, the authorities in Berlin were unable to develop a coherent and consistent policy toward the territories taken from France in 1871. Some urged that the provinces be ruled by the military as befitted foreign territory on the Empire's western frontier. Others hoped to absorb the provinces, to make them German in fact as well as law by a policy of reform, moderation, and conciliation. The conflict between these two views was never resolved. It was usually, but not exclusively, a conflict between soldiers and civilians and thus exemplifies the familiar problem of militarism that persistently flawed the imperial system. There are other familiar problems that emerge from this account of government policy towards Alsace-Lorraine: endemic bureauratic fragmentation and inertia, especially after 1890; inconsistent and self-defeating measures toward the Catholic Church and the labor movement; the inability of political leaders to control the disruptive influences of powerful economic interest groups. The story of the "reluctant union" ends with tentative and incomplete reforms in 1911 that were all but undone by the Zabern fiasco of 1913. In the political aftermath of Zabern the Reich's future could be perceived: military irresponsibility, bureaucratic timidity, and parliamentary weakness combined to produce an appropriate overture for the sequence of events that began at Sarajevo a few months later.

Silverman's account of government policy is lucid and informative. He is somewhat less successful, however, in tracing the political response to these policies within the *Reichsland*. He does show clearly that political alignments in Alsace-Lorraine were complex, generated as much by indigenous religious, social, and economic conflicts as by national and linguistic antagonism. After this book the image of a territory welded together by uniform hatred of the German conqueror must be banished forever to the receptacle reserved for historical myths. But we are not certain what to put in its place. The author states that "by 1914, most Alsace-Lorrainers were willing to become German citizens in the full sense of the word" (p. 3). How is this to be reconciled with the failures of government policy he so carefully records? Was there a process of national integration at work that was perhaps analogous to what Gunther Roth has called the "negative integration" of the labor movement? If so, what does that tell us about the Kaiserreich on the eve of war? How does the situation in Alsace-Lorraine compare to that in southern Germany? How do the problems of the French compare to those of the Poles and Danes? The author gives us a number of fascinating hints about how these questions might be answered, but he never provides the sustained analysis they deserve. Even if we admit that the state of the evidence and the sudden outbreak of war preclude any definitive answers, these issues merit a fuller treatment than is to be found here. In sum, *Reluctant Union* demonstrates but does not exhaust the relevance of Alsace and Lorraine for an understanding of German history during the imperial era.

JAMES J. SHEEHAN
Northwestern University

DANIEL GASMAN. *The Scientific Origins of National Socialism: Social Darwinism in Ernst Haeckel and the German Monist League.* (History of Science Library.) New York: American Elsevier. 1971. Pp. xxxii, 208. $13.50.

Apart from its unfortunate title, this study of Ernst Haeckel's thought and influence enlarges our understanding of what pseudo-science—or "scientism"—has done in the name of science. There were many affinities and some causal connections between the ideas of Haeckel and the realities of National Socialism. But the latter are conceivable without the former, and Haeckel's racism, authoritarianism, militarism, imperialism, male chauvinism, and amoralism would still be bad science and bad philosophy even if Hitler had never become the Führer. Indeed, there is a certain inconsistency in making ideas responsible for something that was so rad-

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ically anti-intellectual, or intellectually so incoherent, as Nazism, and we run the risk of falsifying the nature of fascism when we treat it solemnly as a world view, with philosophic foundations and antecedents and even "scientific origins." Gasman's lucid and perceptive account of how Darwinism was perverted in Germany could easily have stood on its own feet as a devastating analysis of how reactionary ideology managed to masquerade as science; he need not have implied that Germany was uniquely afflicted or that the Nazi tyranny was more than incidentally related to the preachments of Haeckel and his followers.

Having registered these objections, however, I can find little fault with the book. Gasman has made exhaustive use of the literature by and about Haeckel, and he has supplemented this material with some relevant discussion of the activities of other Monists who shared Haeckel's outlook. He thus carries his story down through the Weimar years and beyond 1933, showing that a number of Haeckel's disciples did indeed welcome the advent of the Third Reich. He has a particularly telling section on Hitler's own direct appropriation of Haeckel's conclusions about man, history, society, and morals. Those Monists who followed Haeckel in his rejection of religion but not in his rejection of humanism, internationalism, democracy, sexual equality, and socialism are occasionally mentioned, but Gasman maintains that they were never the dominant tendency in the Monist League and rather implausibly suggests more than once that they were too simple-minded to realize that they had strayed into the wrong camp. This may all be true, but one would like to have more details and a more searching analysis of who joined the league and why. These problems did not loom large in the conceptual framework of the present study, with its focus on the "origins" of Nazism, but our insistent and persistent questions about the cultural, political, moral, and human implications of science would surely justify someone, perhaps Gasman himself, in pursuing them further.

RALPH H. BOWEN
Northern Illinois University

ROLF BAUER. *Österreich: Ein Jahrtausend Geschichte im Herzen Europas.* Berlin: Haude & Spenersche Verlagsbuchhandlung. 1970. Pp. 542. DM 30.

To write a comprehensive history of Austria in a few hundred pages is, by Rolf Bauer's own admission, a demanding task. This professor from Graz has nonetheless accepted the challenge, hoping to provide "all foreign friends of his native land" with an intelligible outline of its development over the previous millennium and to do so without patriotic sentimentality or political bias. In this concise volume he has achieved his essential goals—at least up to a point. Expressing his thoughts in a style that non-German readers will find relatively easy to grasp, Professor Bauer starts out with a brief chapter on Austrian prehistory and takes his subject right up to the year 1970. He covers generally familiar ground, rendering rather conventional assessments despite his intention to compose a volume that would supersede existing surveys. His initial chapters, which appear to lean heavily on a few standard references like those of Erich Zöllner, Karl and Mathlide Uhlirz, and the late-lamented Hugo Hantsch, consist of detailed narratives of political and military events with only occasional, brief discussions of economic, social, and cultural themes. Later chapters, beginning with the one on the reforms of Maria Theresa and Joseph II, are less factual and more analytical. Those dealing with the twentieth century are the most interesting because only then does Professor Bauer offer hints, behind his screen of objectivity, of personal feelings and points of view. In a momentary preoccupation with the problem of the southern Tyrol or a concluding statement on Austria's coat of arms and flags he reveals his natural sympathies in favor of his country and its people.

A few specific criticisms could be leveled at this book. Having organized many of his chapters around single reigns or wars, Professor Bauer has created a framework that does not treat the problem of periodization effectively. The beginning student—for whom the book was ostensibly intended—will have trouble discerning the seminal eras or turning points in Austrian history. Finding himself immersed in a sea of particulars, he might, for example, fail to recognize Austria's emergence as a great power during the late seventeenth and early eighteenth centuries. Similarly, while Professor

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Bauer's thirty-one illustrations, lengthy chronological table, extensive genealogies, and thorough index are to be commended, his complete neglect of maps constitutes a serious weakness in view of his projected audience. The bibliography for a book of this kind must inevitably be selective, but the fact that it is limited, with the exception of a few works in English, to the fruits of German and Austrian scholarship calls into question Professor Bauer's claim to maintaining a balanced judgment. In contemplating the oversights, American readers will probably wonder why the author did not list a solitary title by C. A. Macartney—to say nothing of the historical works that have emanated from the other so-called successor states. Yet, despite such drawbacks, this book constitutes a potentially valuable source for the nonexpert. It contains a great deal of accurate information, presented clearly, succinctly, and with no overriding national prejudices.

JOHN A. MEARS
Southern Methodist University

F. R. BRIDGE. *From Sadowa to Sarajevo: The Foreign Policy of Austria-Hungary, 1866–1914.* (Foreign Policies of the Great Powers.) Boston: Routledge and Kegan Paul. 1972. Pp. xi, 480. $20.00.

If impartiality and thoroughness would suffice to write an outstanding diplomatic history, Dr. Bridge's work should be the definitive volume on the diplomatic history of the Habsburg Empire in the last half century of its existence. Unfortunately the subject demands more. It is necessary also to probe the social and psychological background of events in domestic policies. It is essential to distinguish clearly between the important and the less important and to follow the main themes consistently rather than to give more or less chronological accounts frequently, irrespective of their significance. Finally, the character portraits of the leading protagonists of events and trends call for analysis. In all these respects the author's story leaves much to be desired, though less so in the last third of the volume.

To illustrate the point, the Cretan question receives nearly as much attention as, in the context of the topic, the far more important Macedonian one. Three entirely disparate themes, Austria's participation in the Algeciras conference, the expulsion of Austrian and Hungarian seasonal workers from Germany, and the conclusion of a commercial treaty with Bulgaria, are narrated in two consecutive sentences, and one of them is not pursued any further. This kind of technique makes for neither interesting nor easy reading. Even more important, basic issues like nationality problems, the rift in the dualistic system, and so forth, are at best merely touched upon, and characters essential to the understanding of the story—like Conrad, Francis Joseph, or the heir apparent Francis Ferdinand—are hardly analyzed at all. Francis Ferdinand's influence on the conduct of foreign affairs prior to the appointment of Berchtold as foreign minister in 1912 is underrated, and the old, often-refuted myth that Francis Ferdinand's sympathies for the oppressed Southern Slavs in Hungary were "in the eyes of Serbian nationalists a convincing argument for eliminating him" is once more repeated. Altogether, gossip in diplomatic reports is taken too seriously; for example, a British rumor that in 1911 the emperor considered excluding the heir apparent from the succession on grounds of insanity. That Izvolski, even for the most Machiavellian reasons, was "only too pleased" in March 1909 to comply with Kiderlen's quasi-ultimatum may well be doubted. Yet Dr. Bridge's presentation also includes very sound observations, such as the one that the annexation of Bosnia in 1908 created an upsurge of Pan-Slavist feelings that could not be foreseen by cut-and-dried diplomats like Aerenthal and Izvolski. Equally to the point is the remark that "in the final crisis the solidarity of the Dual Alliance was more the product of mutual distrust" than of loyalty to treaty obligations.

When finally Dr. Bridge raises the question of why the outcome of the July crisis of 1914 was so tragically different from the crises the Danube monarchy had overcome after 1866, he states again rightly, "But the emperor and his foreign minister had always prevailed against warmongers in Budapest and in the General Staff. In 1914, however, the emperor and the minister for foreign affairs joined with the military in opting for war." Why so? According to the author, because "the risks of peace were now greater than the risks of war." What risks,

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one may ask? Dr. Bridge refers not only to the danger of national disintegration but above all to quite a different issue, namely the endangered great-power position of the Habsburg monarchy. This begs the question of what great-power position precisely means and why many an empire in modern history had accepted reduction in power without considering the risks of peace greater than those of world war.

Nevertheless, this work, based on thorough archival research and the use of many—in scholarly quality somewhat uneven—secondary sources, is a serious and respectable study of considerable value.

ROBERT A. KANN
Rutgers University

PETER BURKE. *Culture and Society in Renaissance Italy, 1420–1540.* (Studies in Cultural History.) New York: Charles Scribner's Sons. 1972. Pp. 342. $14.95.

Daring, innovative, and imaginative books on the Renaissance are in short supply. Peter Burke's new work is a fascinating tour de force that will appeal to readers like myself, always on the lookout for new directions in this exciting period of history. It is painful to report that such a book fails, especially since it asks the right questions and proposes so many imaginative methods to get at solutions. The trouble is that the author's erudition is not displayed in a coherent, synthetic fashion. The book is a brilliant failure.

Burke's work tries to build a bridge between cultural and social history through the interpretation of artistic and literary artifacts and personalities, an attempt abandoned for lack of method by Bloch in his *Feudal Society* (1961) and only haltingly ventured by Burckhardt, art historian though he was, in his *Civilization* (1860). The author first posits certain Italian cultural characteristics with the aim of "testing" them in subsequent chapters. They are Burckhardtian: repudiation of tradition, secularism, artistic autonomy (the latter two called "deliberate individualism"), and universality (called "breakdown of compartments"). The first analytical chapter examines the sociology of artists and writers (recruitment, organization, status, etc.), and is based on the statistical evaluation of a 600-strong cultural elite. Following chapters analyze the patron-client relationship, artistic functionalism, taste, and iconography. In the process, scores of insights and legions of facts are marshaled in a manner vaguely reminiscent of Burckhardt. But alas, the facts are often undigested and the repeatedly imaginative aperçus rarely developed. There is no general to order the troops. Burke's essay, unlike his Swiss predecessor's, is not a work of art.

A second section toys with the creation of such a synthetic, artistic whole. In a chapter on world views (which he starts by equating ideology, *Lebensgefühl*, *Weltanschauung*, and *outillage mental*), Burke opines that the age was organic yet occasionally mechanical; calculating but, in a minor key, antirational; active but, then again, contemplative. His categories and insights are often remarkable, but his judiciousness leads to a mechanical pro and con alignment of these categories. The author's tolerance for cultural diversity is unsuited to proposing a synthesis, and he ends all but three pages of this chapter by recognizing that he has run the danger of losing sight of its main aim. In those three pages, he seeks to put a term to the previous chaos and answer the question: "Was there a Renaissance world-view?" Yes, he haltingly suggests, there was a Renaissance "syndrome."

Next the author examines the social framework, with sections on the Church, state, social structure, and, last but not least, the economic and geographical background. He concludes with a chapter on change, hoping to correct the static image he believes he has created. The conclusion of the book is that Burckhardt was partly right and partly wrong, but then again mostly right.

The book is distinguished by openness to new methods, ready use of sociological and anthropological theory, and by cross-cultural comparisons. Burke's willingness to quantify artistic materials and to consider religious phenomena in their universal and not merely Catholic or dogmatic setting heralds, I hope, a new direction in Renaissance studies. Seldom have I written so many "excellents!" in the margin of a book; rarely have I encountered such a cavalier and inadequate use of categories (at one point, the author reduces Weber's three

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types of authority to two on the grounds that "language seems to lend itself to dichotomies rather than trichotomies"). New directions have seldom been atrophied with more panache.

RICHARD TREXLER
University of Illinois, Urbana-Champaign

ANTONY POLONSKY. *Politics in Independent Poland, 1921–1939: The Crisis of Constitutional Government.* New York: Oxford University Press. 1972. Pp. xvi, 572. $24.00.

This book is an excellent example of the obstacles facing the historian of interwar Eastern Europe, especially Poland. The complex, diverse, and often emotional nature of the topic inevitably affects the scholar's outlook, converting him from a detached objective observer into the partisan advocate of a particular viewpoint. While Mr. Polonsky deserves praise for undertaking this ambitious task, the several serious deficiencies of both scholarship and style that mar the book combine with the intrinsic pitfalls in the topic discussed to negate largely his labors. For one thing this work suffers from a marked imbalance of research; although the bibliography includes numerous unpublished source materials, few of them were actually used in the text. Ironically, the same author who criticized contemporary Polish historians rather harshly in the cold-war organ *Survey* (74–75 [1970]: 143–59) drew heavily upon their efforts for his own work, citing published memoirs and secondary sources with notable frequency. This disproportionate use of printed materials and the exclusion of some important, and available, Polish archival holdings are especially evident in the portion of the book dealing with Polish politics from Józef Piłsudski's death in 1935 to the fall of the state in September 1939. Consequently the author appears to have halted his original research at about 1930 and composed the remainder of his work on the basis of a dozen or so memoirs and monographs. He also neglected to use the dispatches of the British and German diplomats in Warsaw, which often contained inside information and penetrating analyses about domestic politics. In fact the absence of a viable analytical framework is another major weakness of Mr. Polonsky's scholarship. Interwar Poland provides an excellent model of a land with a long, illustrious past that emerged from a form of colonial exploitation to confront suddenly the problems of the twentieth century—intensive urbanization, industrialization, social as well as economic modernization—while simultaneously coping with the traditional issues of political power and national sovereignty. The author would have enhanced the value of his work greatly had he selected and developed a theme along this line rather than merely offering token comparative gestures to the Latin American, African, and Asian experiences in the introduction and the conclusion.

In addition to these general shortcomings there are flaws of a more specific stylistic nature. The organization of the book, with its division into topical sections within chronologically arranged chapters, results in a disjointed text that lacks overall coherence. There is also an overabundance of detail, some of it minute to the point of being trivial. Much of this could have been eliminated without loss to the book, whose main points would then have emerged more clearly. Especially distracting are the side digressions on foreign policy, a subject well treated by recent Western as well as Polish historians and hence needing only passing reference at relevant points in what is supposed to be a study of domestic politics. Similar criticism pertains to the footnotes, which are needlessly repetitious and redundant. These deficiencies, combined with lengthy, awkward sentences and a generally ponderous, stilted style, make the book difficult reading for a specialist in the field, let alone a scholar not thoroughly familiar with Polish affairs. Finally, the tone of Mr. Polonsky's writing appears overly subjective, even strident in places, thereby raising doubts about his credibility as a serious scholar. A minor strength are the detailed statistical tables in the appendix, but these, too, are largely based on figures from the early 1920s. While careful editing should have eliminated most of the numerous printing errors and remedied the stylistic lapses, the author must bear final responsibility for the finished product. In this instance, it is a disappointment.

EDWARD D. WYNOT
Florida State University

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Istoriia mist i sil Ukraïns'koi RSR [History of the Cities and Villages of the Ukrainian SSR]. *Ivano-Frankivs'ka Oblast'* [Ivano-Frankivska Province], edited by O. O. CHERNOV *et al.*; *Kyïvs'ka Oblast'* [Kiev Province], edited by F. M. RUDYCH *et al.*; *Mykolaïvs'ka Oblast'* [Nikolaev Province], edited by V. O. VASYL'EV *et al.* (Holovna Redaktsiia Ukraïns'koi Radians'koi Entsiklopediï Akademiï Nauk URSR.) Kiev: Institut Istoriï Akademiï Nauk URSR. 1971. Pp. 639; 790; 771.

With the exception of the Crimea, where complicated nationality problems pose an especially difficult problem for Soviet historiography, all of the urban, industrial regions of the Ukraine were covered in volumes of this encyclopedic work appearing prior to 1971 and discussed earlier in these pages (*AHR*, 76 [1971]: 1570–73 and 77 [1972]: 546–47). The three volumes listed above constitute a good cross-section of the remaining fifteen provinces.

Kiev province is the predominantly rural region left after the "union-republic" level city of Kiev is detached as a separate unit of administration. The rich history of this region is reflected in the volume: accounts of the refounding of scores of villages as the Mongol wave receded; passing reference to Polish-Lithuanian "oppression" and Tatar raids; and abundant details on landownership and rural social stratification in the last decades of the tsarist period. Treatment of the Soviet period, especially the trauma of collectivization, is less satisfying. But there is a mass of contemporary material not contained in published census results (for 1959 or, it would seem, for 1970), economic handbooks, and Communist party reports. The careful reader will also gain an unusual insight into the continuity of rural conditions—for example, from the way in which authors of a small minority of the local accounts boast how their towns and villages (usually only a few dozen miles from Kiev) are connected to the capital by "asphalt" roads.

Like other southern provinces, Nikolaev does not have the rich, nearly continuous history of the Kiev region. Still, there are interesting accounts of Cossack-Tatar conflicts on the Black Sea borderlands and occasional detailed references to the origins of settlers arriving in the area after permanent Russian rule was established in the late eighteenth century. I noticed, however, only one instance (Berezanka, formerly Alexanderfeld) where the complex history of the numerous German settlers (who fled or were banished at the end of World War II) is treated.

The volume on the Ivano-Frankov province provides information of an entirely different order. This mountainous west Ukrainian area, acquired by the Soviet Union in 1939, was the principal locus of the resistance of the Ukrainian Insurgent Army, the strongest and most protracted anti-Soviet guerrilla movement. This volume contains a surprising amount of information on the UPA (referred to by the Soviet authors as the "Ukrainian bourgeois nationalists," "OUNists," or "Banderists"). About half of the fifty extended local accounts mention UPA activity between 1944 and 1950. In several cases the accounts indicate that the insurgents killed from twenty to thirty Soviet "activists"—sizable figures, considering that today party membership in these localities is usually between fifty and a hundred. The accounts make it clear that the security police and their local auxiliaries constituted the principal counterinsurgent force; lack of participation of regular army formations (after the end of World War II) accords with other information we have. It is interesting to note that the authors list several "Chekists," numerous Komsomol secretaries, and many heads of village governments among the UPA's "victims"—but no district party secretaries, who evidently enjoyed better protection. Much more information on this significant resistance movement could be obtained by village-by-village comparison of these Soviet accounts with the detailed versions of UPA activity that appeared in the emigré press in the late 1940s and early 1950s.

JOHN A. ARMSTRONG
University of Wisconsin, Madison

W. E. D. ALLEN, edited with introduction, additional notes, commentaries and bibliography by. *Russian Embassies to the Georgian Kings (1589–1605)*. Texts translated by ANTHONY MANGO. In two volumes. (Works issued by the Hakluyt Society, Second Series, Numbers 138 and 139.) New York: Cambridge University Press for the Society. 1970. Pp. xxxii, 368; ix, 369–640. $18.50 the set.

Immediately after the appearance of Russian forces on the Caspian in 1556 with the annihilation by Ivan IV of the khanate of Astrakhan,

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the ruler of Kakheti, the easternmost of the survivor states that resulted from the fragmentation of the Christian kingdom of medieval Georgia, sought the support of the Orthodox tsar against the constant threat of Kakheti's Muslim neighbors. Not all the records of the earliest relations between King Alexander II of Kakheti and Ivan IV preceding the first Russian embassy of I. D. Rusin in 1586 have survived, but between 1564 and 1605 no less than seventeen embassies moved back and forth between Moscow and the Georgian courts as the Russians sought to extend their sphere of influence southwestward from the estuary of the Volga and to insert themselves into the complicated political pattern dominated by Ottoman Turkey and Safavid Persia, as well as a number of lesser Muslim principalities, which prevailed in the area.

These early Russian attempts ultimately proved premature. The control of Caucasia remained in Turkish and Persian hands, and even the expeditions of Peter the Great a century later failed to establish Russia permanently on the Black Sea or to drive a wedge into the Caucasian mountains. The feeble Georgian principalities remained the vassals of their Muslim masters. Nevertheless, the accounts of these abortive missions are of profound interest for the scholars of the period. As noted by the editor, the accounts provide "particulars of Russian daily life and administrative practice" and "an intimate picture of life and customs among the peoples of the northern and central Caucasus and in Georgia—which is not available for the period in any other source," as well as "the first extant accounts of the crossing of the main chain of the Caucasus from north to south" (vol. 1, p. xviii). As such, they are invaluable to geographers and ethnographers as well as historians.

The first summary of the embassy records was provided in 1844–45 by the great specialist in Georgian history, Marie-Felicité Brosset. The Russian texts for the period 1578–1613 were published in the all but unobtainable edition of S. A. Belokurov, *Snosheniia Rossii s Kavkazon* [*Russian Relations with the Caucasus*] (Moscow, 1889). For the present translation the editor selected from Belokurov's *Relations* the account of two significant missions—that of Semen Zvenigorodski and Torkh Antonov (1589–90) and that of Mikhail Ignatyevich Tatishchev and Andrey Ivanov (1604–05). To these Allen joined extracts from the Sovin and Polukhanov mission (1596–99). The texts are accompanied by an ample introduction, notes, extensive commentaries on numerous aspects of interest, a discussion of the archival and published sources, supporting genealogical charts, and an excellent bibliography. Reproductions of a number of maps, many of them rare, and equally unfamiliar, attractive illustrations, some of them in color, complement this handsome publication.

The long labors of Messrs. Allen and Mango, the translator, have given us a valuable insight into this insufficiently known material. The translation is both precise and as readable as possible, within the limits of the stiff bureaucratic prose of the Moscow chancery. Mr. Allen's command of the minutiae of Caucasian and Georgian history eminently qualifies him to provide the indispensable elucidations and commentaries. Indeed, so great is his acquaintance with the region and its history that he may at first overestimate that of his reader. Even experts in the perils of Caucasian scholarship may find their path among Avars, Kumukhs, Kumyks, Chechens, Ingushs, Shapsughs, Shevkals, and so forth, let alone Georgian intrigues, as precarious as the Terek route across the Daryal gorge traveled by the ambassadors; less-experienced scholars may lose their footing altogether. Yet perseverance will eventually resolve most of the initial difficulties and clear a way through this enormously intricate subject.

Historians may regret occasional inaccuracies and omissions. Astrakhan was captured in 1556 rather than 1554. The chronology of the kings of Kartli (vol. 1, p. 56) is less accurate than in the tables of volume 2 (p. 590). The deletions of "excessively long accounts of negotiations" tend to be disturbing, despite the assurance that they are "of very minor importance" (vol. 1, p. xix) and despite the careful delineation of their length. Certain technical terms such as *dyak* are neither translated nor sufficiently explained, and some transliterations are debatable. In view of the paramount importance of the geographical setting, the usefulness of the illustrative maps is greatly reduced by the absence of any indication of relief. The index—where entries such as the Circassian

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Adighe and Shapsugh are missing altogether, while the Shevkal are first listed on page 36 although they appear on pages 4 and 6–7 of the introduction—requires greater accuracy, and the addition of an index for the multiple technical terms would have been most welcome. Despite these minor lapses, however, Mr. Allen has rendered a great service to Western scholars in putting such valuable material within their reach. We can but hope that his example will be followed in making available still more of the rich diplomatic and travel accounts, generally beyond the reach of investigation, without which the history of the period remains perilously incomplete.

NINA G. GARSOÏAN
Columbia University

JOSEPH T. FUHRMANN. *The Origins of Capitalism in Russia: Industry and Progress in the Sixteenth and Seventeenth Centuries.* Chicago: Quadrangle Books. 1972. Pp. viii, 376. $12.50.

This book began as a dissertation on the Russian iron industry in the sixteenth and seventeenth centuries. For purposes of publication it has been dressed up under the pretentious title of *The Origins of Capitalism in Russia.* The author has tried to ponder the meaning of "capitalism" and in the process "came to respect Karl Marx for his keen understanding of the vast and complex historical phenomenon which historians refer to as the 'transaction [*sic*] from feudalism to capitalism.'" Yet his lucubrations have yielded only sparse results. "The manufactory [as distinct from the factory] system which took shape in Western Europe after the fourteenth century . . . *was* capitalism." Russia in the sixteenth and seventeenth centuries developed forty-seven enterprises using water power that "may therefore without controversy be considered manufactories." In his conclusion, however, he notes that "*serfdom* was the main barrier to extensive capitalist development in Russia during this period," and he refrains from saying that capitalism existed there at all.

In the process of rewriting, Dr. Fuhrmann has added several chapters packed with the usual citations from secondary sources that he apparently refers to as his "own research." Unfortunately these chapters, dealing with minor industries other than iron and with the activities and status of foreigners, were written, as the author puts it, in the hope "to broaden the appeal of my study," obviously without regard to their lack of relevance to the problem of capitalism.

There remains the original body of matter relating to the iron industry, in the compilation of which Dr. Fuhrmann apparently combed the available secondary literature in Russian and in foreign languages. The result is a jumble of biographical and anecdotal material, largely ignoring technological matters. Indeed, he seems not to have mastered the actual technology, permitting statements such as "it was wasteful to cart large amounts of slag over long distances," leaving the reader to wonder why anyone should wish to transport slag at all. Perhaps he means, not slag, but crude ore; even so, it does not explain his statement in another passage that the English shipped large quantities of Russian ore to be smelted in England.

The whole is provided with appendixes, extensive notes, a glossary, a "note on sources," a "selected bibliography," and an index with two sketch maps to show the location of industrial activity.

JESSE D. CLARKSON
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WILLIAM F. WOEHRLIN. *Chernyshevskii: The Man and the Journalist.* (Russian Research Center Studies 67.) Cambridge, Mass.: Harvard University Press. 1971. Pp. 404. $12.50.

This is the first comprehensive monographic study of the life and writings of N. G. Chernyshevskii (1828–89) to appear in English. Heretofore this problematical and uniquely influential figure of the Russian Revolutionary tradition has been made to occupy chapters in broad interpretive works or has been the subject of brief essays; all in marked contrast to the Russian-language literature on Chernyshevskii, which is voluminous.

The book is described by its author as an "investigation of Chernyshevskii's writings in the setting of a relatively detailed account of his life. The study makes no claim to be either a sociological analysis or a psychological portrait." It is made up, in effect, of three parts.

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Two are biographical, dealing, respectively, with Chernyshevskii's life from early youth through his career as editor of Nikolai Nekrasov's *Sovremennik,* which Chernyshevskii helped to make the most popular "fat journal" in Russia in the late 1850s and early 1860s, and with an account of his political activities of the early 1860s, his arrest (July 7, 1862), trial, imprisonment, and exile until his death. The other part, which separates the two biographical parts, analyzes Chernyshevskii's thought, dealt with under rubrics corresponding to the main subjects of his writing as a journalist and scholar ("philosophy," "aesthetics and literary criticism," "economics and social theory," and "politics and the theory of revolution").

The job the author has set for himself is executed in an intelligent and scholarly way: the corpus of Chernyshevskii's writing has been carefully reviewed; the scanty evidence about his political activities outside legal journalism has been judiciously weighed in the light of analysis of his political thought; and the enormous pre- and post-1917 literature on Chernyshevskii has been read and exploited. (The prospective reader may wish to take note, however, that the considerable production of *Chernyshevskiana* since 1965 is not reflected in this book; there are only two titles published after 1965 in the bibliography, both American publications.) On the many puzzling aspects of Chernyshevskii's thought and deeds Professor Woehrlin almost invariably hews to the most reasonable interpretation among those that have been proferred over the sixty-odd years of Chernyshevskii scholarship in Russian. The work can be recommended confidently to the non-Russian reader, whether he is in search of a general study of this major figure or merely in need of an account of Chernyshevskii's views on one or another question.

It is doubtful, however, that the scholar already familiar with Chernyshevskii's more important writings and with the Russian-language scholarship will learn much from this book, although he may find rewarding the author's cautious wading-about in the swamp of extravagant claims that have been made concerning Chernyshevskii's "conspiratorial activities" of the early 1860s. Chernyshevskii was neither a systematic thinker nor a revolutionary organization man; it was the novel style of his thought and the novelty of the objects of his concerns that made him such a central figure in the Russian Revolutionary movement. We need to know more about the psychological and Gestalt elements that produced this peculiar, and prototypical, pattern of characteristics; and it is just these areas of inquiry that Woehrlin either deliberately eschews or passes over with a few phrases.

TERENCE EMMONS
Stanford University

NEAR EAST

CHARLES ISSAWI, editor. *The Economic History of Iran, 1800–1914.* (Publications of the Center for Middle Eastern Studies, Number 8.) Chicago: University of Chicago Press. 1971. Pp. xv, 405. $17.50.

In the last two decades the development of Iran's economy has been extraordinarily rapid. The rate of real growth "has averaged 7 to 8 percent, and the per capita growth nearly 5 percent, figures matched by barely half a dozen countries." Industrialization, urbanization, land reform, the expansion of transportation facilities, and increased social mobility have produced tensions and created problems upon the solution of which will depend the continuation of the nation's progress over the next quarter century.

Professor Charles Issawi, a perceptive observer of Iran's economic development, has put together an anthology that is appropriately entitled *The Economic History of Iran, 1800–1914,* since in this case the whole is definitely more than the sum of its parts. In the opening pages Professor Issawi gives much needed geographic and historical background. The rest of the book is divided into a general introduction and eight chapters that deal respectively with social structure, foreign trade, transportation, agriculture, industry, and petroleum. The epilogue surveys the Iranian economy from 1914 to 1970. Each chapter consists of contemporary documents, extracts from diplomatic and consular reports, passages from travel accounts, and analyses by later writers. Most of these appear in English for the first time and many are not easily obtainable. The arrangement of the material is such that the

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book can be read consecutively, Professor Issawi having provided enlightening introductions and commentaries that tie the excerpts into a comprehensive and digestible whole.

The documents and other excerpts constituting this book show a country deep in social and economic depression. Whether one reads the dispatches of General Gardane, Napoleon's envoy to Fath Ali Shah, or English reports on the Persian Gulf trade, Russian railway projects or Persian descriptions of domestic highways, commercial statistics or literacy figures, all the materials confirm the impression that nineteenth-century Iran was a poor, stagnant, and nearly hopeless nation. The vast land was virtually roadless, the masses were ignorant, the small sophisticated elite was selfish, the government was incredibly corrupt, and foreign influences were largely negative. One does not have to argue the question of whether it is the economy that causes social backwardness or vice versa. The facts are eloquent: Persia was by all standards among the most backward nations of the world. Moreover, her economy underwent only minor changes until the reign of Reza Shah. Even the discovery of oil and the establishment of the oil industry made little difference until the 1920s when the government began to make the first conscious efforts to induce both social change and economic growth.

The low state of the economy partly explains the political impotence of the later Qajar Shahs. It also explains the small amount of foreign investment and the predominance of political and strategic factors in the Anglo-Russian rivalry in Persia. Professor Issawi's book will undoubtedly become indispensable not only to those who are interested in Iran but also to all who wish to understand the impact of the West upon other societies.

F. KAZEMZADEH
Yale University

AFRICA

YVONNE TURIN. *Affrontements culturels dans l'Algérie coloniale: Écoles, médecines, religion, 1830–1880.* ("Textes à l'appui.") Paris: François Maspero. 1971. Pp. 434.

In this rich, well-documented volume Yvonne Turin presents a superb, original contribution to the understanding of the dynamics of Algeria's early colonial history. *Affrontements culturels* deals with three interwoven topics: the elaboration and implementation of French educational and public health policies in Algeria between 1830 and 1880; Muslim Algeria's responses to those same policies; and an analysis of the quality of the cultural confrontations inherent in this interaction. Since, as in most societies at a given stage, education and medicine in precolonial Algeria had been adjuncts of religion, the struggle tended early to become one between a theocentric world view on the one hand and a rationalist, secularizing one on the other. In this connection the author sensitively reminds us that the struggle is rendered more urgent by virtue of the fact that the colonizers themselves, sons of the Enlightenment and the Revolution, were still acutely aware of the struggle within their own society that rationalizing forces were waging against the last bastions tions of *ancien régime* traditionalism. According to Turin, modernization in a colonial context differs in intensity and perceived discontinuity, but not in kind, from the internally inspired modernizations of European societies.

The student of French imperialism who has been won over to the facile generality that the *mission civilisatrice* always and everywhere entrained a heavy-handed imposition of purely French culture, values, and education will be surprised by this book. Professor Turin maintains that during their first fifty years in Algeria the French did not consciously try to impose their culture. Perceptive French civil servants and soldiers, recognizing the impossibility of rapidly Europeanizing a culture so passionately convinced of its own superiority, sought primarily, first, to upgrade somewhat the quality of traditional education in the *kuttâbs, zawâyâ,* and *madâris* and, second, to create in the *écoles arabes-françaises* centers of a truly bicultural education to be dispensed to Arab and European children alike. With the twin exceptions of vaccination programs and the state *madrasas,* all French efforts to involve Muslims in French educational and health plans met with massive, tenacious, and durable resistance. The result of almost fifty years of effort was almost complete failure to achieve the goals the colonialists had set. Attempts to change the traditional schools were abandoned in the fifties and sixties. Bicultural schools, badly staffed

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and usually underattended, succumbed next. The Muslim Algerian, for whom education was neither more nor less than a means to acquire knowledge of religious truth and practice, was rightly suspicious of French efforts to tamper with those means. The French saw education as rationalist and utilitarian and as a vehicle for lessening Algerian opposition to French occupation. The latter aim was also perceived by the Algerians, who saw no reason to abet it. As traditional education languished and bicultural schools died, the field was left by default to the purely French *écoles communales*, which, by virtue of the individual choices of a few Algerian families beginning in the third occupied generation, would become the principal instruments of Gallicization.

Mlle. Turin's study was researched primarily in the Archives nationales, the Archives de la Guerre, and the Archives d'Outre Mer. Students of colonial Algeria recognize the problems and pitfalls presented by this lopsided documentation. If at times the author seems carried by her documentation to overly detailed description of the internal mechanisms of colonial management, she uses the same sources with considerable skill as a mirror to reflect the mind and movement of the subject population. If Turin is guilty of any major fault it is perhaps in not emphasizing sufficiently the overwhelming impact of the colonial presence itself upon the evolution of education. Years of warfare, progressive eviction of populations from the most fertile lands, and heavy-handed confiscation of *habous* so impoverished Muslim society that while on the one hand a small group of administrators and soldiers was trying to improve education, the resources through which the society could afford any kind of education at all were being rapidly and permanently denied it.

JOHN RUEDY
Georgetown University

ASIA AND THE EAST

B. S. JAIN. *Administration of Justice in Seventeenth Century India (A Study of Salient Concepts of Mughal Justice)*. With a foreword by A. L. SRIVASTAVA. Delhi: Metropolitan Book Company. 1970. Pp. xii, 153. $4.00.

NOMAN AHMAD SIDDIQI. *Land Revenue Administration under the Mughals (1700–1750)*. New York: Asia Publishing House for the Centre of Advanced Study, Department of History, Aligarh Muslim University. 1970. Pp. 182. $7.00.

J. S. GREWAL. *Muslim Rule in India: The Assessments of British Historians*. New York: Oxford University Press. 1970. Pp. xvi, 218. $4.50.

R. K. PARMU. *A History of Muslim Rule in Kashmir, 1320–1819*. Delhi: People's Publishing House. 1969. Pp. xiv, 544. Rs. 40.

Historians of South Asia have saddled themselves with a peculiar adaptation of the European periodization "ancient," "medieval," and "modern" in which "medieval" denotes that time period during which there were states in north India whose rulers were Muslims. "Modern" history is assumed to begin with British rule in Bengal in the mid-eighteenth century. Thus the books under review all fall into the "medieval" category as taught in Indian universities, a periodization now justified solely by methodological convenience. Most, but not all, of the Muslim rulers of this period made Persian their official language of record, and this continued until the East India Company switched to English in 1837. These four books have in common the assumption that a "medieval" historian is one who reads Persian and that the medieval period is that for which Persian (and other) sources are valuable.

Jain's study of Mughal law courts represents an older style of scholarship, for his is a library thesis using printed sources exclusively—and even among those relying heavily on foreign travelers' accounts, English translations rather than Persian originals, and secondary sources. The largest collection of relevant primary materials, the *Fatawa-i-Alamgiri*, is mentioned only in passing. A chapter on important cases discusses only thirteen examples, all from the descriptions of the European travelers Manucci and Tavernier. One example at the *parganah* (local) level in eighteenth-century Bihar is offered as the only evidence that certain procedures were typical throughout India in the seventeenth century (p. 83). Jain twice cites an example from Baghdad in Abbasid times to illustrate the powers and procedures of an Indian official in Mughal times (pp. 90, 93), and his whole section on the *muhtasib* (censor of morals) is written from extra-Indian examples and sources. His assumption that legal theory and

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practice in the classical Arabian caliphates governed Mughal theory and practice is questionable without some Indian evidence. Jain's conclusion that Mughal justice was on the whole fair and quick, free from the modern preoccupation with technicalities, may be true, but it is not proven here.

In contrast, Siddiqi's study of agrarian relations in the Mughal heartlands in the eighteenth century is a masterful example of the excellent scholarship emanating from the "medievalists" at Aligarh Muslim University. Siddiqi's study of the three-cornered relationship between government, landlords, and peasants is firmly based upon an exhaustive study of land revenue documents in the Uttar Pradesh State Archives at Allahabad, plus contemporary manuals of administrative procedure, Mughal news reports, and chronicles. Useful comparisons with other regions of India enhance Siddiqi's discussion of developments in Awadh, the central Gangetic plains. He follows eighteenth-century usage in distinguishing the peasants as that class of cultivators without the right to sell or mortgage the land they tilled, as distinct from cultivating proprietors and landlords proper whose fields were tilled by others. Government action is described at both the imperial and the provincial and local levels, along with the role of the hereditary local officers. Siddiqi's study demonstrates the vicious circle in eighteenth-century India of inflation of official ranks and salaries pressing revenue demands upwards, encouraging the spread of *ijara* or tax-farming, and the result of stagnation in the rural economy and a weakening of the machinery of imperial government.

The energetic Punjabi scholar J. S. Grewal has demonstrated his own mastery of primary source materials in his two collaborative volumes with B. N. Goswamy on Mughal religious policy. In the present volume Grewal analyzes the development of British historiography of medieval India, which developed the notion of the "Muslim period" as, unfortunately, we know it today. But Grewal's study is more than an exercise in the history of history, for he demonstrates that his historians, from Alexander Dow in the 1760s to Mountstuart Elphinstone in the 1840s, were directly and intimately connected with the development of British rule in India. Their interest in India was closely connected with their views on policy. The worst, like James Mill, seemed to feel that accurate knowledge of the Indian past was unnecessary; Grewal quotes Mill's view that "a residence in India or a knowledge of the languages of India, was, to express myself moderately, not indispensable" (p. 93). Mill's history reflected this unconcern for basic data. Grewal's study does not show any consistent development over the eighty years but rather a consistency between the attitudes towards Indian culture of each writer and the quality of his history. Sir William Jones ("It is my ambition to know India better than any other European ever knew it" [p. 49]), John Briggs, and especially Elphinstone had a deep and sympathetic understanding of Indian culture that enabled them to use Indian source materials effectively. Grewal's epilogue on later British historians argues that this pattern continued, some scholars mastering primary sources, others disregarding them out of a racial arrogance that encouraged the belief that no document written by an Indian in an Indian language could be a reliable source of historical data.

Radha Krishna Parmu's book is a narrative political history of Kashmir over the five-hundred-year span of Muslim rule with some information on socioeconomic and cultural developments. Thus it surveys the entire "medieval" period during which this region was brought into full participation in the affairs of the subcontinent. Parmu has utilized a wide range of sources—literary, epigraphic, and archival—in Sanskrit, Persian, Kashmiri, and English, informed by his own liberal regional patriotism. His general account is not likely to be superseded, but it can be questioned and improved in detail. Parmu's main weakness is his failure to explain the conversion of the Kashmiris to Islam, the most important historical process in this period, although to be fair to him contemporary evidence on this vital topic is lacking in all parts of India. His claim that incorporation into the Mughal Empire imposed stagnation might be questioned—the presence of Kashmiri artists and the famous *Katib* (calligrapher) Mulla Muhammad Husain Kashmiri in Akbar's royal atelier, for example, indicates that the empire offered a wider scope for Kashmiri talent outside the valley in compensation for the predominance of non-Kash-

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miris in the political administration of the province.

Three of these four volumes on "medieval" India offer encouraging hope that Indian work in this field, although restricted to a few institutions and individuals, will be of an increasingly high quality. With the earlier monographs of Athar Ali, Satish Chandra, Irfan Habib, and S. C. Misra we see developing a new approach to medieval studies, informed by a more thorough examination of relevant primary materials than was the fashion before independence. Pakistani scholarship has apparently been vitiated by slavish adherence to the "two-nation" theory, but despite the horrible examples of P. N. Oak and some textbook writers, historical research in India has not suffered from the constraints of any official or sectarian view of the Indian past.

FRITZ LEHMANN
University of British Columbia

PETER STUART. *Edward Gibbon Wakefield in New Zealand: His Political Career, 1853–4.* [Wellington:] Price Milburn for Victoria University of Wellington. 1971. Pp. 195.

Probably the greatest merit of this little volume is that it is not founded, as are most biographical treatments of Wakefield's life, on his own highly misleading account of the important events in which he took part. Fortunately there were other literate and intelligent settlers in Christchurch, Wellington, and Auckland who kept journals and wrote letters and were not unduly biased in Wakefield's favor. Pre-eminent among them was Henry Sewell who went out on the same ship as Wakefield to wind up the affairs of the Canterbury Association, who for their first months together retained his high opinion of Wakefield as an enlightened and original thinker, but who was gradually disillusioned by Wakefield's efforts to assume a dictatorial role in the New Zealand Parliament and especially by his betrayal (as most of his colleagues saw it) of those principles on land policy for which he had always stood. There seems little doubt that this metamorphosis in the Wakefield gospel was due to his desire to win a constituency in the Hutt valley where the larger part of the voters was landless and (in a good many cases) unemployed laborers. That a change in land policy was needed is probably true, but Wakefield made the efforts of Governor Grey in that direction the main count in Wakefield's violent attack on him.

The most interesting part of the book is the account of the meeting of the first New Zealand Parliament in Auckland. Wakefield was able to pose successfully as the man who had done most to introduce "responsible government" in Canada by persuading the governor general, Sir Charles Bagot, to accept the Baldwin-Lafontaine government. On this point he led astray not only New Zealanders of his own day but also the author. Canadian historians have shown conclusively since the Bagot and Draper papers have been available that Bagot's conversion was due to pressure from his conservative ministers, especially Draper, who forced him to come to terms with Lafontaine and his followers. But if Wakefield had had surprisingly little influence on Canadian politics, he did for a time acquire real leadership among his colleagues in Auckland, and this tale of his last pyrotechnic display of his political and oratorical gifts makes fascinating reading.

HELEN TAFT MANNING
Bryn Mawr College

UNITED STATES

CLARENCE H. CRAMER. *American Enterprise: Free and Not So Free.* Boston: Little, Brown and Company. 1972. Pp. 728. $17.50.

Dean Cramer, no doubt from the notes of a lifetime, has written a series of very readable and often penetrating essays centering around American economic history. Such writing, however, can gain greatly in value if it acquaints the reader with what is new in relevant secondary sources, which this book in all of its seven hundred pages fails to do. To fit this omission of thirty years of scholarly work in business history the punishment should be to read the *Business History Review* and the *Journal of Economic History* from their beginnings.

THOMAS C. COCHRAN
University of Pennsylvania

JOHN UPTON TERRELL. *American Indian Almanac.* New York: World Publishing Company. 1971. Pp. xiv, 494. $15.00.

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This book is limited to the Indians in the territory that is now the United States, minus Alaska and Hawaii. It makes only a few brief references to Indians of Mexico, Central, and South America. "The authoritative reference and chronicle" claimed on the dust jacket is a masterpiece of overstatement. I prefer to label the book a good amateur effort rather than a bad professional one, but it is much less authoritative than a half dozen other secondary sources on Indians of North America or the United States. The term "almanac" also suggests a more encyclopedic treatment than is the case.

The book divides the United States into ten prehistoric or early historic culture areas. This scheme does not match very closely the archeological culture areas of Willey (1966), or, for the early historic period, the areas of Spencer and Jennings (1965), or of Driver (1961, 1969), to mention only three sets done by professional anthropologists. Each of the ten parts begins with a summary of the archeological data, which take up from about ten to thirty per cent of the space, and then proceeds to describe the historic tribes in the area one at a time. The chronological chart of the archeology, inside both the front and back covers, is at best only a rough match to the more authoritative chronological charts in Willey (1966).

The descriptive detail throughout the book is fairly accurate, although sometimes distorted by language intended to glamorize the writing for the general reader; but glaring errors appear now and then. For instance, "intertribal warfare, incessant in all regions" (p. xiv) is a gross exaggeration. Indians in California and the Great Basin generally, and some tribes in the Southwest, gave little attention to war. "There is no doubt that domestic corn was developed in South America" (p. 12) flies in the face of archeological discoveries of earlier corn in the state of Puebla, Mexico, which appeared in print more than a decade before this book was published.

The selected bibliography on the whole is a good one but fails to include the following authoritative secondary sources that would have been a great help to Terrell in organizing his material: Jennings (1968), Spencer and Jennings (1965), Spicer (1962), and Willey (1966). The glossary near the end is a useful one, and the index of fifteen double-columned pages facilitates reference to the material.

I would recommend the book to the general reader interested in Indians but not to the professional historian or anthropologist to use as a work of reference.

HAROLD E. DRIVER
Indiana University, Bloomington

WILCOMB E. WASHBURN. *Red Man's Land/White Man's Law: A Study of the Past and Present Status of the American Indian.* New York: Charles Scribner's Sons. 1971. Pp. viii, 280. $7.95.

JENNINGS C. WISE. *The Red Man in the New World Drama: A Politico-Legal Study with a Pageantry of American Indian History.* Edited, revised, and with an introduction by VINE DELORIA, JR. New York: Macmillan Company. 1971. Pp. xiv, 418. $8.95.

FRANCIS PAUL PRUCHA *et al. American Indian Policy.* (Indiana Historical Society Lectures, 1970–1971.) Indianapolis: the Society. 1971. Pp. 65. $1.50.

WILBUR R. JACOBS. *Dispossessing the American Indian: Indians and Whites on the Colonial Frontier.* New York: Charles Scribner's Sons. 1972. Pp. xiv, 240. Cloth $7.95, paper $3.95.

SIEGFRIED VON NOSTITZ. *Die Vernichtung des Roten Mannes: Dokumentarbericht.* [Düsseldorf:] Eugen Diederichs Verlag. 1970. Pp. 157.

These five volumes are considerations of various aspects of the status of the North American Indian in a white-dominated culture, particularly his legal and social status. In this they continue a line of investigation begun ninety-two years ago by the amateur historian Helen Hunt Jackson (*A Century of Dishonor* [1881]) and more recently reactivated by Angie Debo (*A History of the Indians of the United States* [1970]) and Vine Deloria, Jr. (*Of Utmost Good Faith* [1971]). The volumes by Wise and Washburn are histories of the evolving sociolegal status of the Indian from the time of contact to the present. Jacobs's study is difficult to characterize since it is a collection of essays gathered from various journals; essentially his theme seems to be the cultural significance to both whites and reds of the dispossession of the latter. Von Nostitz seeks to introduce German readers to the long-term historical phenomenon of the conquest. The lectures by Father F.

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P. Prucha, William T. Hagan, and Alvin Josephy, Jr. that comprise the Indiana Historical Society Lectures for 1970–71 deal with the origins and consequences of governmental Indian policy.

On balance, I would recommend Washburn's book as being the most informative for both the professional historian and the general reader. Its self-stated purpose is to describe the "process by which the Indian moved from sovereign to ward to citizen" (vii). After a too-brief opening section, "Theoretical Assumptions," in which he alludes to the philosophical and intellectual underpinnings of Western civilization's exploration and settlement of the New World, Washburn moves into a strong and deftly rendered historical survey of the constantly altering power relationship between the defenders and the invaders and of the legal justifications supplied by the latter in order to bring the process of dispossession within the pale of civilization. What Washburn's survey reveals is that insofar as the Indian has been concerned American law has been based on power rather than on justice, and that what has been styled "Indian policy" has been little else than the random afterthoughts of a nation's felt needs. Two passages from Washburn contain the essence of all this. The first is Theodore Roosevelt's blunt articulation of the national need: "this great continent could not have been kept as nothing but a game preserve for squalid savages" (p. 38). The second is Washburn's own description of Indian policy, a description his survey fully confirms: "a strong expression of interest by the Congress based on a hasty assumption about what was good for the Indian and a more calculating assumption about what was good for the white; the policy phrased in rhetoric evoking images of the Declaration of Independence and the Sermon on the Mount; instructions to the executive to carry out the policy on pain of financial cutbacks or administrative extinction; and an unspoken assumption that the Indians could be cajoled, forced, frightened, or persuaded into recognizing the benevolent intent of the framers" (p. 85).

Wise's book is a reprint of one issued more than forty years ago. Appropriately, it now bears the impress of the native hand, for Vine Deloria, Jr. has revised and edited it and supplied brief but valuable end-matter that sketches the Indian's changing status from the Indian Reorganization Act (1934) to the Red Power movement. Unfortunately, Mr. Deloria did not revise and edit enough, for too much of this large, passionate book is either misleading or plainly inaccurate. Moreover, an ugly strain of anti-Catholic bigotry leads Wise into some outrageous blunders, such as his claim that the legendary founder of the League of the Iroquois was a Jesuit missionary and that the creation of the league was a part of a huge papal conspiracy. Less glaring but perhaps more serious is his statement that "about one hundred of those engaged in the [Minnesota Sioux] massacre of 1862 . . . were executed" (p. 264). The actual number is thirty-eight. His treatments of Geronimo and of the Ghost Dance movement are similarly irresponsible, and Mr. Deloria must share the blame for letting all these go unchallenged. There is much to be said for reprinting neglected works of Americana but nothing to be said for letting older errors go unacknowledged. What does deserve reprinting here are chapters 13–21 wherein Wise delineates the moral and legal precedents for the Removal Act. He brings to this his background as legal counsel for the Yankton Sioux, and his discussion rightly focuses on the legal struggle of the Cherokees in the 1820s.

This is about where Father Prucha picks up the chronicle. In "The Image of the Indian in Pre–Civil War America" he argues that despite certain unfortunate errors and oversights government Indian policy has always been informed by humane equalitarianism and a sincere desire for the Indian's ultimate welfare. This is indeed a lonely position and one requiring some intellectual courage. I wish I could admire Father Prucha's conclusions as much as I do his courage, but it seems to me that such defenses of Andrew Jackson and others as he has been making over the past several years are precisely those being made in our time of the men who have mapped and prosecuted government policy in Southeast Asia. The other essays in this volume by William T. Hagan, "Indian Policy After the Civil War: The Reservation Experience," and Alvin Josephy, Jr., "Toward Freedom: The American Indian in the Twentieth Century," are much less

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controversial. Both are instructive and straightforward, though I question Professor Hagan's conclusion that in the cultural climate of post–Civil War America there were no realistic alternatives to the reservation system. What about the setting of permanent and generous boundaries for the tribes beyond which white settlement could not occur? This is unrealistic only to a morally enfeebled government such as ours had shown itself to be in the conflict between Georgia and the Cherokees, but it is not unrealistic, and it seems to me that Professor Hagan ought to have said so.

Wilbur R. Jacobs in *Dispossessing the American Indian* and Siegfried von Nostitz in *Die Vernichtung des Roten Mannes* consider the consequences of official and unofficial Indian policy. Both books suggest in different ways the truth of Hawthorne's observation that guilt is a very heavy burden. *Dispossessing the American Indian* is, as noted above, not really a book with a coherent theme, but if it were it would be an anthropologically oriented study of the meaning of cultural destruction and ecological despoilment. It is unfortunate that Professor Jacobs has not succeeded here in bringing cultural and physical anthropology to the service of historiography. There are probably few historians more knowledgeable of the facts of Indian-white relations on the frontier, and this subject urgently requires an angle of vision that would supply us with the cultural significance of the facts Jacobs and others have unearthed.

Von Nostitz makes the inevitable and awful comparison between governmentally sanctioned genocidal practices in America and Germany. There is nothing new in his book, however, since it relies heavily on standard histories of Indians and whites. If there is any comfort to be drawn by his German readers from his comparison, there can be precious little for Americans.

FREDERICK W. TURNER, III
University of Massachusetts, Amherst

DAVID J. WEBER. *The Taos Trappers: The Fur Trade in the Far Southwest, 1540–1846.* Norman: University of Oklahoma Press. 1971. Pp. xiii, 263. $8.95.

Scholars have long analyzed the fur trade and told and retold the exciting adventures of the mountain men who trapped the Western beaver streams. Good studies have been written of several companies engaged in this enterprise. But this is the first detailed study of the Southwestern trappers, men who moved in and out of Mexico's northern frontier and made Taos their winter home. David Weber ferreted out from scarce, unused materials found in Mexican archives a fresh overview and a new perspective of the New Mexico fur trade.

The efforts of the Spaniards to profit from their northern frontier placed a premium on the pelts and hides obtained mainly through barter with the Indians. In fact, these skins "probably constituted New Mexico's chief export item at the end of the Spanish period." Inevitably the French from Louisiana and, after 1803, the Americans competed with the Spaniards. Despite danger from both Indians and Spanish officials, these intruders continued to plunder Taos country streams that abounded with beaver.

It was not until after 1821, when the Mexicans won their independence from Spain, that American trappers and traders were able to profit from the furs of the southern Rockies. New Mexico officials abruptly reversed Spanish policy by allowing foreign merchants to enter New Mexico villages. Within the first year several groups of Americans successfully trapped through the Sangre de Cristos. Fur prices were rising and a flurry of activity followed. Despite the red tape and taxes imposed by the central government in Mexico City, by 1842 trappers were working out of Taos and Santa Fe into the Colorado Basin, the Gunnison River, and the Green River country.

The author traces the tribulations of both companies and individual trappers until the American conquest of New Mexico in 1846. He underscores the endurance of these mountaineers as they spread throughout the West to California. In particular, Weber emphasizes the many clashes with Mexican officials. Although they were often victims of changing policies, the Americans also suffered because they trapped furs illegally and attempted to smuggle them to markets. In many cases the furs were confiscated, ruined in waterlogged caches,

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or stolen. A fall in the demand for beaver and a boom in buffalo hides brought a gradual decline in beaver trapping in the 1830s, and by the mid-1840s the fur trappers had nearly disappeared.

The author does not attempt to enthrall the reader with the legends and stories that arose from the adventures of these mountaineers. The names that abound in each chapter will undoubtedly turn off most general readers. But for the scholar interested in Western or New Mexico history this is an interesting, important, and carefully researched study.

JIM B. PEARSON
North Texas State University

PATRICIA WATLINGTON. *The Partisan Spirit: Kentucky Politics, 1779–1792*. New York: Atheneum, for the Institute of Early American History and Culture. 1972. Pp. viii, 276. $12.95.

Patricia Watlington's well-written study of Kentucky politics between 1779 and 1792 gives the reader a concise description of the confused political situation in the district and raises some important questions about early political development in American frontier areas. She postulates the existence of three political groups or "parties" whose machinations and conflicts shaped the early politics of Kentucky. The first party, the "Partisans," was composed of large speculators and landless settlers and had little attachment to Virginia. This party at first wanted a separate state, established by Congress, with guarantees that would protect their shadowy titles, which had not been granted by Virginia, or would give them opportunities to acquire land. When these early efforts failed they shifted their position and opposed a separate state that they feared would be controlled by their political rivals. These rivals at the beginning consisted of one group that the author terms the "articulate center." Composed of surveyors, place holders, and owners of large Virginia grants, this group at first opposed any separation from Virginia. Then during the 1780s it divided into a "court" party composed of judges, lawyers, merchants, and manufacturers who pushed for separation from Virginia even at the risk of secession from the United States and a "country party" of planters and farmers who favored a legal separation from Virginia and continued union with the new nation. The author uses these three groups to describe and explain the activities of the numerous Kentucky conventions, the bitter partisan rivalries, the activities of General Wilkinson, the negotiations with Spain, and the formation of Republican and Federalist factions. Her study of political differences in this frontier society suggests that other frontier states and territories instead of being single party units also possessed factions and parties that competed for land, power, and place.

Yet despite the excellent narrative and the generally clear argument the book suffers from two weaknesses. The first, and perhaps a relatively minor one, is her overemphasis on the ignorance of earlier historians of these bitter factional disputes during the 1780s. Thomas D. Clark in *A History of Kentucky* (1937) discussed the division between the court and country parties, and although he overlooked the partisans he certainly did not describe early Kentucky politics as a one party system. The second weakness seems more important to me. It is, quite simply, that the author never really systematically relates her three parties, either in terms of leaders or supporters, to the developing economy, social structure, or governmental institutions of the new district. The author, for example, states that commercial and manufacturing interests tended to support the court party, but there is never any detailed description of the persons or areas involved with this interest and their relations with that party. The same can be said about place holders, militia officers, large and small landholders, landless settlers, and so on. There is also a lack of analysis of the various conventions in terms of how delegates from various regions and representing specific interests actually responded to various programs. Thus the book is really based upon the activities and writings of a very small elite group of visible politicians, and it seems that the author fails to relate their activities to the interests and desires of the larger population.

Despite these criticisms the book is by far the best narrative study of politics in an American frontier district in the post-Revolutionary period, and as such it must be read by any Ameri-

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can historian interested in the political development of the early United States.

VAN BECK HALL
University of Pittsburgh

ROY V. SCOTT. *The Reluctant Farmer: The Rise of Agricultural Extension to 1914*. Urbana: University of Illinois Press. 1970. Pp. xi, 362. $8.95.

For the past forty years, Alfred C. True's *A History of Agricultural Extension Work in the United States, 1785–1923* has served as the standard work on the subject. It has been supplemented by the more specialized writings of Joseph C. Bailey, Gladys Baker, Wayne C. Neely, and Roy V. Scott on the national or multistate level of American agricultural history as well as by the publications of scholars working on specific states.

Roy V. Scott's volume on the rise of agricultural extension to 1914 provides a second comprehensive account of this nationwide educational movement. Although his study is also encyclopedic in detail and scope, it is superior to True's in organization and interpretation. In addition Scott has incorporated into his monograph the literature of the subject that has been published since True's work appeared in 1928.

Scott begins his narrative with a survey of the educational efforts of innovative farmers, agricultural societies, and rural fairs. Next he turns to the contributions to adult education of the Patrons of Husbandry and the Farmers' Alliance. His theme then becomes the search for the most effective teaching device for rural men and women. He discusses in turn the farmers' institute movement, agricultural extension by land-grant colleges, educational work by railroads and other business corporations, the Southern demonstration program of Seaman A. Knapp, and the employment of county agents in the North. In Scott's opinion the federal Smith-Lever Act of 1914 was the culmination of the quest for the most desirable method of mass education among farmers.

The author covers all aspects of the growth of agricultural extension to 1914 and summarizes developments in virtually every state and territory, as well as within the United States Department of Agriculture. His research and documentation are massive, and his scholarship is painstaking. At the same time he never loses sight of his basic thesis, the evolution of a successful extension-teaching technique.

The book might more appropriately have been titled "Some Reluctant Farmers" or even "Some Reluctant Administrators." Throughout the narrative appear examples of progressive farmers who were in advance of the scientists in the search for improved agricultural methods. As Scott correctly states in his second chapter, organized husbandmen steadfastly demanded that reluctant land-grant colleges and state boards of agriculture enter the field of agricultural extension. Even the county agent, who usually earned the confidence of the country dwellers, was not infallible.

WILLIAM D. BARNS
West Virginia University

JOHN B. BOLES. *The Great Revival, 1787–1805: The Origins of the Southern Evangelical Mind*. [Lexington:] University Press of Kentucky. 1972. Pp. xiii, 236. $10.00.

This study of the Great Revival is based on meticulous and critical research and is well written. It attempts to explain why such a religious revolution occurred, adding to the conventional interpretation "the feeling of crisis" that arose at the close of the eighteenth century. The personality of an obscure but remarkable man, the Presbyterian minister James McGready, seems to have been the catalyst that precipitated the movement in Kentucky which then spread by contagion through reports of the "work of God," into the other Southern states. Mr. Boles rejects the frontier interpretation of the movement. Nevertheless, to me frontier social conditions seem to have had great influence in promoting this emotional religious phenomenon. This study recreates a past epoch of the history of the South—so different from today—when a conviction of sin convulsed a large part of society.

The author pays scarcely any attention to the lurid and sensational aspects of the Great Revival, with its vast amount of noise, its screaming and shouting, its trances, the "jerks," the "holy laugh," and so on. Instead, he seeks to understand the theology of the preachers, their methods of arousing an audience to emotional frenzy, and the effects of the

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revival on the Southern mind. The Great Revival was confined largely to the Methodist, Presbyterian, and Baptist denominations, but for a brief period, 1800–05, the denominational distinctions were submerged. Mr. Boles makes one of his main points that there was an underlying unity in the theology of these churches. Although much has been made of the resort to frightening people into religion at this time, he points out that a study of the contemporary sermons shows that they emphasized the love and mercy of God to those who repented. A fundamental emphasis also was placed on "conversion," which required a dramatic individual experience. The revival promoted the idea of the millennium to be inaugurated by Christ through his return to the earth. The author finds that the ministers appeared to be relatively unconcerned with economic or political theory or the social gospel but overwhelmingly concerned with the salvation of the individual soul. The movement led to a democratization of religion, to strengthening ascetic tendencies of the region, and to schism—especially over the Calvinistic doctrine of election that caused the formation of the Cumberland Presbyterian Church and the Christian Church. The author concludes that the Great Revival had far-reaching effects in firmly establishing orthodoxy of mind in the South not only in respect to religion but in respect to preserving slavery.

CLEMENT EATON
University of Kentucky

SILVIO A. BEDINI. *The Life of Benjamin Banneker*. New York: Charles Scribner's Sons. 1972. Pp. xvii, 434. $14.95.

The achievement of Benjamin Banneker, known as the first black scientist in the United States, must be measured by his modest contribution to eighteenth-century natural philosophy and the obstacles he overcame. Grandson of an Englishwoman who was transported to Maryland as a convicted thief and a freed slave who claimed to be an African prince, son of their daughter and a former slave from Guinea, Banneker spent his seventy-five years as a farmer and small landowner in Baltimore County. He turned to the serious study of astronomy in his late fifties with the encouragement of the Ellicotts, a neighboring white family of successful entrepreneurs skilled in technology. Building on a knowledge of elementary mathematics acquired as a schoolboy and using borrowed books and instruments, Banneker pursued his interest to the point where he could carry out the tedious calculations for a twelve-month ephemeris, the heart of the almanac.

Banneker came to the attention of abolitionists in Maryland and Pennsylvania. Recognizing his usefulness as an example for countering the argument that blacks were inherently inferior, they encouraged him in his work and helped find publishers for what became his popular series of almanacs for the years 1792 through 1797. Banneker's reputation as the "sable astronomer" spread through the republic. Jefferson described him to the Marquis de Condorcet as "a very respectable mathematician" and "a very worthy & respectable member of society" (p. 159). Appointed assistant to the surveyor of the site chosen for Washington, Banneker made some of the astronomical observations important in the preliminary mapping.

After his death in 1806 Banneker and his accomplishments were largely forgotten except by those interested in the antislavery cause or the condition of blacks in America. His reputation suffered from neglect and exaggeration. The biography by Silvio A. Bedini now changes this. Using new sources he brings Banneker into focus as "a man of modest ability and performance, who, by means of his efforts, contributed a tangible bit to the fabric of science in America" (p. xiv). Bedini deals effectively with the early history of agriculture and industry in tidewater Maryland and with the details of eighteenth-century astronomy and almanacs. In the search for Banneker the scientist, however, the author loses Banneker the man.

The situation of a black freeman in Maryland could not have been easy, but Bedini claims that "because of the relative remoteness of his home, work, and leisure from any community, Banneker had little if any experience of oppression and cruelty to Negroes" (p. 150). Yet the evidence Bedini presents argues otherwise. Banneker, for example, noted in his journal several thefts of his possessions and threats against his life, including the discharge of a shotgun at him while he stood in the doorway

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of his house. Furthermore, Bedini indicates that Banneker had a drinking problem serious enough to attract public notice. In three of the dreams Banneker recorded he saw himself dead and wandering through hell "seized with horror," wrestling with "an illformed being—Some part of him in Shape of a man, but hairy as a beast," and holding in his arms a child with an old wound penetrating skull and forehead (pp. 331–35). Finally, as Banneker was lowered into the grave a few yards from his house the building caught fire and burned to the ground. Bedini avoids the dark undercurrents suggested by these incidents. Although he ventures to describe on little or no evidence what Banneker thought or felt at particular moments, Bedini does not face what may be central themes in the scientist's life. Neither does he offer a detailed and comparative evaluation of Banneker's contribution to science and culture in post–Revolutionary America. The reader must work that out alone—not an easy undertaking even for the knowledgeable. Nevertheless the book will long remain the standard reference for this remarkable man not only because it is the first major study of him, but because of the author's careful research and the more than sixty pages of documents and critical bibliography he appended.

HAROLD FRUCHTBAUM
Columbia University

ALEXANDER DECONDE. *Half Bitter, Half Sweet: An Excursion into Italian-American History.* New York: Charles Scribner's Sons. 1971. Pp. vii, 466. $12.50.

LUCIANO J. IORIZZO and SALVATORE MONDELLO. *The Italian-Americans.* (The Immigrant Heritage of America Series.) New York: Twayne Publishers. 1971. Pp. 273. $6.50.

Half Bitter, Half Sweet does not primarily (or simply) examine Italian emigration, immigration to the United States, or the assimilation process, although these topics are included. The book discusses a wide range of subjects in American-Italian relationships from the eighteenth century to the 1960s, with the purpose of studying and placing in perspective the relations between Italy and the United States, conditions and activities within both countries, and contacts between Americans and Italians. DeConde is at his best when he discusses international relations, attitudes of American travelers toward Italy and of their Italian counterparts toward the United States, and American reactions to immigrants from Italy. Of great value is the volume's extensive attention to the period since the 1920s, which comprises somewhat more than half the text.

DeConde should succeed in his aim of reaching a wide audience; however, his examination of a vast range of topics, shrewd insights, coverage of areas seldom touched on by books focusing on Italian-American history, and a fifty-nine-page bibliographical essay will interest scholars. The bibliography in itself should attract anyone concerned with immigration history and ethnic studies.

This book is the best overall examination yet written of the Italian-American experience. Not a definitive study, its main weakness is its attempt to cover all possible topics; of necessity, some areas receive insufficient treatment. Occasionally DeConde accepts traditional views that ought to be closely scrutinized, such as the one that Italian-Americans lag in intellectual, cultural, and social achievements in comparison with other groups. According to him, Italians progressed but should have accomplished more. If he intends to contrast Italians with Jews, Irish, and Germans he is undoubtedly correct. If, however, DeConde has in mind a comparison with other large contemporaneous groups from Southern and Eastern Europe (with the obvious exception of Eastern European Jews), the viewpoint is questionable.

Although tourism has formed a significant element in the new Italian prosperity there is, inexplicably, no mention of the importance of the tourist trade in the otherwise excellent overview of the post–World War II Italian situation, a discussion that includes American business influences, social planning, and education, cultural exchanges between the two countries, the Sicilian Mafia, and the return of emigrants to Italy.

The book contains some factual errors. *L'Italia,* for example, was not the first Italian-language newspaper published in Chicago (p. 75), and Chicago's first Italian alderman was elected in 1885 not 1892 (p. 94). In the overall view, such details are of minor importance, but one wishes that they did not occur.

It is difficult to determine the audience that

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The Italian-Americans was designed to reach. This is the first volume published in the Immigrant Heritage of America series, which is dedicated to "the contribution made by specific ethnic groups to the development of America." This being so, the book needs an introduction by the series editor explaining its focus and how it fits into the series framework. Iorizzo and Mondello present material that could provide supplementary information for introductory college history and high school social studies classes, but the book's heavy emphasis on upper New York State and New York City suggests a regional market; however, the extensive footnoting seems to be aimed at scholars rather than regional, general, or high school–college markets. On the other hand, the text presents little that is new for the specialist, except for its handling of the *padrone* system in the small cities of upper New York State and the treatment of Italians in popular magazines of the Progressive period, topics that the authors have examined in previously published articles.

In contrast to DeConde's broad treatment Iorizzo and Mondello concentrate on Italian immigration. Their intention is to present a comprehensive treatment of the immigrant experience; the volume's brevity, a condition probably imposed by the publisher, produces uneven results. A long chapter discusses the rural experience, but no chapter deals specifically with the development and growth of Italian ethnic colonies in the "burgeoning cities" where the bulk of the immigrants lived. The book attempts to present the small-town experience using Oswego as a representative location, but gives no indication of the factors making that town representative of small urban centers throughout the country.

This volume had great potential and the admirable intentions of its authors. It needed a firm and consistent editorial hand.

HUMBERT S. NELLI
University of Kentucky

CHARLOTTE ERICKSON. *Invisible Immigrants: The Adaptation of English and Scottish Immigrants in Nineteenth-Century America.* Coral Gables, Fla.: University of Miami Press. 1972. Pp. vi, 531. $17.50.

This is the best edition of immigrants' letters since Thomas and Znaniecki published *The Polish Peasant* fifty years ago. Taken from manuscript rather than the usual edited newspaper versions, the twenty-five series by English immigrants (no more than two are Scots) are mediated only by Dr. Erickson's perceptive introductory chapters.

She is candid about the limitations of generalizing from this single category of sources. The Englishman (seldom here a woman) who was given to writing home was apt to be an isolated individual, remote because of occupation from the clusters of his compatriots in the larger cities and industrial centers; the latter seem to have felt less need for letter writing. But among those who did—the farmers, artisans, small entrepreneurs, and marginal professional men of this collection—the mails afforded something of the same reassurance, both practical and nostalgic, that the urban ethnic community furnished its members.

Dr. Erickson's new sources do not, indeed, undermine the older interpretations, which have largely been based on data from the ethnic communities, as often as she asserts they do. Like immigrants generally, most of the letter writers had fled a loss of social status, threatened by modernization of the British economy, while they still had the capital to re-establish their families in the relatively backward agriculture or industry of America. Believers, down to 1850 if not later, in the same "agrarian myth" of independent proprietorship—whether of a farm or a house and garden—that led Americans to migrate, they were "social conservatives" who valued the self-employment, secure subsistence, and modicum of leisure that might be found where land was cheap, more often on the land-office frontier than behind it. Although widely dispersed thereby, they further resembled the urban joiners of ethnic churches and lodges in "accommodating" to American society without being wholly "assimilated" into it. As a Suffolk man wrote from Penn Yan in 1873, "Mericke" provided "plenty of hard work," but "whee prefard Englent to live."

The freshest ground broken here concerns the role of the English family in both emigration and adaptation. The letters are pre-eminently family documents. When they contained advice for intending emigrants, the latter were relatives—specifically not, as historians have imagined, miscellaneous newspaper readers and

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public-house habitués, troublesome as all these would be if they were to turn up on the family doorstep in America. Letters sometimes enclosed remittances to bring a mother or brother over; more often they requested additional capital to help establish the family's overseas branch; throughout, they kept up family solidarity despite the miles and years. (It is significant, though not as paradoxically as Dr. Erickson supposes, that those individuals who most successfully assimilated to nineteenth-century American society came from broken families and, when they did write home, mingled effusive sentiment with acrimonious legal or moral claims against parents, brothers, or sons.) The ordinary English immigrant letter writer belonged to an extended family, in the practical, unsentimentally old-fashioned sense that it served as a mutual-assistance association, though centered not on a peasant holding in the old country but on one or more households in America, permanently fixed in place as long as the emigrant generation lived. The letters of such families moreover imply little conflict between the generations. This is a substantial contribution to the history of American society at large as well as to immigration history.

ROWLAND BERTHOFF
Washington University

PETER R. KNIGHTS. *The Plain People of Boston, 1830–1860: A Study in City Growth.* (The Urban Life in America Series.) New York: Oxford University Press. 1971. Pp. xx, 204. $7.95.

This remarkable little book embodies two important paradoxes, one generic to much quantitative work in the "new urban history," and the other more particular to Knights's unique contribution to such work.

The first involves all those ways in which modern historians, in order to reconstruct the experience of the masses of ordinary people, use new methods that necessarily distance the reader from the vividness, drama, and personality of those newly visible lives; those who are the building blocks of history "from the bottom up" can seem as unreal in the flood of charts and tables as they have seemed invisible in the smooth narrative of traditional elitist history. Readers must therefore bring to books like this appropriate expectations and seek the richness and humanity in easily overlooked tables and statistics. In an important sense, the content of such work lies more in the method than in the story it tells or the conclusions it produces.

This leads to the more particular paradox of *The Plain People of Boston,* a book that is at once thoroughly timid about generalizing from its data but that at the same time is boldly challenging in the way its method forces a revision of our most basic modes of thinking about urbanization, no small accomplishment for only ninety pages of text, supplemented by sixty-nine charts, five appendixes, and three maps.

Knights studies the movements of a large sample of Boston's "ordinary people" through social and physical space between 1830 and 1860, looking for patterns in this mobility and for its role in the more encompassing process of city growth. His book's uniqueness lies in its treatment of geographic mobility, and it will probably take on the importance for subsequent study of this phenomenon that Thernstrom's early work has had for the study of social mobility. By tracing a sequence of decade sample groups through manuscript censuses, city directories, vital statistics, tax rolls, and even cemetery lists, Knights can say with precision who came to Boston and for how long, who left, and what characterized the remarkably general residential movement of those who stayed. Dividing the city into functional core and periphery areas, Knights identifies no less than eight forms of spatial movement, all intertwined and almost all, significantly, invisible in aggregate census data. This done, he explores how different patterns and sequences correlate with various social and economic population groupings.

Such correlations, if fully articulated and demonstrated, would clearly carry enormous significance for getting at the hidden dynamics of urban social history, and it is in this sense that Knights has made only an exploratory start. His material on social mobility is the least effective; working with admittedly superficial categories and qualified data, he finds few dramatic relationships between spatial and social mobility. His more exacting work with the more demonstrable categories of native and foreign-born yield more promising results, for he discovers numerous contrasts in both loca-

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tion and sequence of movement, contrasts that suggest new ways of understanding how urban space mediated the relationship of these groups to industrialization and to each other.

Proudly drawing support from Holmes (Sherlock not Oliver Wendell), Knights refuses to speculate beyond the reach of his tentative data. He has been criticized for this, but it seems to me a wise choice, for such speculation would mean little until more refined categories are employed. Rather than the artificial core and periphery, for example, we could look at specific neighborhoods and link social patterns to their structure and history—including, as Knights suggests, such crucial variables as industrial and commercial locational change. Furthermore, in refusing to outrun his evidence, Knights actually helps to focus on what is really instructive about his work; it is job enough to attack the notion of an urban population that, in a misleading organic metaphor, "grows" and to demonstrate the liberating effect of the metaphors of flowing, blending population currents and the "churning" residential patterns they produce.

Knights, to be sure, has not invented such notions or originated all the tools demonstrated in his book. But he has put together a comprehensive, understandable, and provocative example of an exciting new approach that, if it cannot be casually read as urban history, must certainly be studied and absorbed with great profit by anyone interested in the field.

MICHAEL FRISCH
State University of New York, Buffalo

MICHAEL H. FRISCH. *Town into City: Springfield, Massachusetts, and the Meaning of Community, 1840–1880.* (Harvard Studies in Urban History.) Cambridge, Mass.: Harvard University Press. 1972. Pp. ix, 301. $10.00.

"Community" is an alluring but elusive concept. Its analytic potential and problems for urban historians are both evident in Michael Frisch's study of Springfield, Massachusetts, between 1850, when it was a "town" of twelve thousand, and 1880, when it had become a "city" of thirty-three thousand. "By presenting the details of urban history with an eye to their conceptual implications," Frisch proposes "that the transition from town to city can be understood as a human and cultural process, that it will be possible to explore how quantitative changes in condition and organization are related to qualitative changes in values and culture."

In the second chapter, "The Meaning of Community," Frisch weaves together the interpretations of Zuckerman, Baltzell, Thernstrom, Gusfield, Benson, Banfield and Wilson, Weber, Williamson, Sydnor, and Katz in support of his interesting but undemonstrated premise that there was "a genuine sense of community" in pre–Civil War Springfield. The larger purpose here, however, is the statement of his thesis: "What was to happen in Springfield in the following decades can be described in its essential outline as the reversal of this conceptual situation: the divergence of public and private [interests], the decline of community as a cultural reality and its emergence as an abstracted concept."

In the following chapters, Frisch describes this transition through a detailed discussion of the economic, political, and physical development of the city. There is an acute analysis of the debate over municipal incorporation, the psychological impact of the Civil War, economic innovation and entrepreneurship, the changing shape and appearance of the urban environment, and the expansion of municipal responsibility and institutions. Frisch is particularly good on the period of "self-examination" during the 1870s when a policy of "retrenchment" completed this cycle of growth; this is interpreted as a rejection, not a reform, of expanding institutionalized power and a local manifestation of Wiebe's "crisis of the communities."

Despite Frisch's appearance in the new Thernstrom and Tilly series, his appreciation of the work of many social historians, and his implicit acceptance of the importance of social structure, Frisch emphasizes the "qualitative changes in values and culture" within his analytic equation at the expense of the "quantitative changes in condition and organization." Because the analytic context is restricted, however, the conceptual implications are limited. Frisch's interpretation of the complex relationship between public and private interests is imaginative and important: in many respects it

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refines and expands Warner's notion of "privatism." But Frisch's conclusion that cultural influences were prior to and distinct from social conditions in Springfield is unproven, and, therefore, his contention that "the emergence of an abstracted conception of community" is central to the understanding of urban history is unpersuasive.

WALTER S. GLAZER
University of Pittsburgh

ALBERT FEIN. *Frederick Law Olmsted and the American Environmental Tradition.* (Planning and Cities.) New York: George Braziller. 1972. Pp. xi, 180. $10.00.

In 1968 Albert Fein edited *Landscape into Cityscape: Frederick Law Olmsted's Plans for a Greater New York City,* an anthology of park reports. In his latest book he offers a sixty-nine page essay accompanied by eighty pages of illustrations to show that Frederick Law Olmsted was as wide-ranging and important in his accomplishments as Benjamin Franklin, Thomas Jefferson, and William Penn and that he was one of the first to insist that we must do the physical planning of our living environment in harmony with natural ecology.

Olmsted's books, articles, reports, and letters reveal a vigorous interest in everything from scientific agriculture to the rights of women, religion, and the problems of an industrial society. His *Cotton Kingdom* (1861) was a classic account of the effects of slavery on Southern society. Olmsted was the organizing genius of medical relief for the Union armies during the Civil War. Afterward he was the farsighted planner of twenty great urban parks, following the magnificent example of Central Park, which he and Calvert Vaux had designed in New York in 1858. Until the close of his career in 1895 he designed many suburbs such as Riverside, Illinois, and campuses like Stanford University's; and he did his final work on Biltmore, the huge domain of George Vanderbilt in North Carolina.

Fein divides Olmsted's life into two phases. During the period from the 1850s until the late 1870s, Olmsted—like his fellow countrymen—worked for the social betterment of all Americans. After the hopes for social justice were dashed by the failure of Reconstruction and the rise of big business, Fein argues, Olmsted—again like his countrymen—retreated to a largely private practice of landscape architecture in which esthetic rather than social concerns were emphasized. The multimillionaire was Olmsted's new client, replacing the middle-class professionals and merchants who had supported Olmsted's democratic social ideals. Fein contends that from the 1880s on, Olmsted worked in his profession merely as a consultant; he was no longer a paid official such as he had been in New York, responsible for carrying out his plans and responsive to the needs of the public.

Fein's generalization about Olmsted à la Richard Hofstadter is helpful but not conclusive. While it enables Fein to do justice to Olmsted's concern for the plight of the Southern slave and the farm laborer in England, it allows him to neglect Olmsted's concurrent delight in the English landscape. Though a theoretical enthusiast for liberal democracy, Olmsted was also something of an autocrat. He was never a happy subordinate. To face down both the politicians and his wealthy private clients, he was eager to establish his authority as a professional landscape architect, especially after his break with his partner Calvert Vaux in 1872. There was to be no "user participation" in the design process once Olmsted's employment had been agreed upon. He defended his work against such revision over and over again. In spite of his later work as a consultant, he continued to do justice to social as well as esthetic considerations in his recommendations.

Fein's enthusiasm for Olmsted's social thought leads him to point out, for the first time, the fruitfulness of Olmsted's partnership with the publicist Jonathan Baxter Harrison, to whom he delegated much of the campaign to preserve Niagara Falls and the Adirondacks from commercial exploitation and whom he sent on a follow-up trip through the South to see how much progress had been made there since the ending of slavery. Fein perhaps stretches things a bit to try to establish a link between the thought of Olmsted and Edward Bellamy or Lester Frank Ward.

Fein's summary of Olmsted's principles of design is admittedly an extrapolation of Olmsted's ideas into present-day modes of thought.

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Certainly his work with geologists, engineers, and public health officials on the Staten Island Improvement Plan was a step in the 1870s toward the interdisciplinary cooperation we would like to see in the ecology movement today. Fein's emphasis obscures the fact that Olmsted thought of the landscapes he created out of derelict wastelands around our cities as unified works of art. Although he used some native plant materials in Boston's polluted fens and the desolate rocky spine of Manhattan, the principles he was following were those of the English picturesque, not ecology. Therefore he was not prevented from using more exotic materials whenever he chose. However admiring we can be of Olmsted, we must understand that he deliberately manipulated nature to produce esthetic effects refreshing to the harried city dweller of his day rather than to the passing migratory bird.

In conclusion Fein has given us an enthusiastic, but hardly subtle, thumbnail sketch of Olmsted's career. He is surprisingly vague about Olmsted's originality in design—no mention, for instance, of the separation of roadways and paths in Central Park. One would assume that the eighty pages of photographs and plans intended to form a separate pictorial essay would remedy this defect. Unfortunately many of the illustrations are poorly reproduced and presented with little or no explanation of their significance. We wind up with an anthology of plans, useful to the specialist, but difficult for the laity. Victoria Ranney's *Olmsted in Chicago* (1972) and Elizabeth Barlow's *Frederick Law Olmsted and New York* (1972) do a less complete but far more comprehensible job of illustrating Olmsted. Perhaps a straining for originality and relevance prevented Professor Fein from meeting the standard of objectivity, balance, and informativeness he so beautifully achieved in his essay "The American City: The Ideal and the Real," published in Edgar Kaufman's *The Rise of American Architecture* (1970). Despite the fact that it covers a broader topic than the book under review, Fein's earlier essay remains one of the best appraisals of Olmsted in print because it makes a persuasive synthesis of Olmsted's social thought and his design accomplishment.

CHARLES C. MCLAUGHLIN
American University

HUGH MARSHALL, S.T. *Orestes Brownson and the American Republic: An Historical Perspective.* Washington: Catholic University of America Press; distrib. by Consortium Press, Washington. 1971. Pp. vii, 308. $13.95.

The remarkable characteristic of the work of Orestes Brownson, who died in 1875 embittered by the Republican betrayal of the great moral mission of America, is the union of a patriot's dream with analysis of current affairs capable of revealing to his readers, in his own time and ours, many of the most basic streams of American social and political development. The present study, while primarily an exposition of Brownson's work rather than a study of its adequacy as interpretation of his times, is both thorough and competent. Brownson believed that the mission of America was a grand social and political experiment in the union of human possibilities with divine and natural laws, and he died convinced of his country's failure. Categorically and independently Catholic, Brownson's sense of American destiny was Puritan at heart; his disillusionment parallels that of the Mathers as they watched their own divine experiment fall victim to commercialism and decline of spiritual vision.

Marshall traces the development of Brownson's thought only by uniting its major periods with its central preoccupations: to name the peaks, constitutionalism, secession, the contest for the Union, Reconstruction, and the destruction, as Brownson saw it, of constitutionalism in Republican radicalism. The organization of the book is fair. While a reader might hope for a closer integration of Brownson's thought with the main currents of political thought from the Revolution until 1875, the author may be expected at a further level in the evolution of his own scholarship to do significant work in that larger sphere. Brownson's Catholicism is discriminatingly dealt with, both in its limitations and its idealism; and the author is highly objective in revealing the contingent, not to say, idiosyncratic aspects of Brownson's most earnest convictions.

ELWYN SMITH
Eckerd College

RICHARD N. ELLIS. *General Pope and U.S. Indian Policy.* Albuquerque: University of New Mexico Press. 1970. Pp. x, 287. $10.00.

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This book is not a biography; it elucidates the views of General John Pope on Indian policy during two decades beginning with the period of the Civil War. Pope, as well as other military men, is credited with having been more humane in his attitude toward Indians than some humanitarians, and he is said to have initiated ideas that led to the Peace Policy of the Grant administration.

This is correct only in the sense that the general wanted to place Indians on reservations and simultaneously subject them to the influences of education and Christianity. In every other respect Pope favored military control of Indian affairs, and Ellis argues that the War Department was more capable of feeding the Indians, as well as of supervising Indian assimilation, than was the Interior Department. In this regard the book lacks objectivity for, contrary to the author's opinion (pp. 236–37), improvements were made in the system for supplying Indian agencies during the 1870s, and the appointment of petty Eastern politicians through political patronage was the exception to the rule. Nowhere is a distinction made between the Peace Policy years from 1870 to 1878 and the periods that preceded and followed them in this regard.

Ellis correctly points out that Pope had other than humanitarian motives in wanting to improve the system for feeding Indians on reservations. If they were adequately supplied, there would be no need to allow the tribes to hunt, in which case they could be disarmed, thus eliminating raids on white settlers. As long as Indians possessed the means of making raids, it was impossible for the army to satisfy its Western critics. For the same reason Pope wanted the authority to pursue hostiles within the reservations. Military logic moved from the premise that the army was responsible for making transportation links and settlements secure to the conclusion that Indian affairs should be conducted by the War Department.

In other respects Pope's ideas for improving Indian administration were unrealistic, and it seems likely that a promilitary perspective has obscured this fact. To advocate the removal of Indians to eastern locations "within or behind the settlements" where they could be fed and prepared for assimilation at low cost was impractical in view of public opinion. Likewise, Pope's advocacy of stock raising instead of farming as a means of subsistence for nomadic Indians was good in theory, but it did not take into account the increasing pressure upon pasture lands as the plains were invaded by white cattlemen. Furthermore, the Dawes Land in Severalty Act provided for the allotment of reservations for the purpose of grazing, as an alternative to farming, where the environment was better suited to ranching.

Despite these lapses, Ellis has clearly portrayed Pope's views on Indian policy and the scholarly literature is richer for his effort. It is, however, regrettable that the book lacks balance concerning the merits of military versus civilian administration of Indian affairs and that the personality of General Pope does not emerge in its pages.

HENRY E. FRITZ
St. Olaf College

PAUL T. DAVID. *Party Strength in the United States, 1872–1970.* Charlottesville: University Press of Virginia. 1972. Pp. xiii, 310. $9.75.

This book presents state-by-state percentages of Republican, Democratic, and "Other" votes for president, governor, senator, and cumulated congressional races, plus three indexes of party strength for even years from 1872 to 1970. The indexes are averages of real and extrapolated percentages for all four offices (index A), all but the president (index B), and the House and Senate (index C). Brief introductory chapters discuss indexes, rules for fitting various party labels into the three categories, and some generalizations drawn from the data.

Some will regret that this valuable reference work reports only percentages, so one cannot distinguish turnout from preference effects, and that every odd-year election has simply been listed under the following even year. More information could have been put on the same number of pages using a line for each year, with an entry for raw vote total and extrapolated figures indicated as such by italics.

For those who use this book, several caveats are in order. First, there are some errors in the data, which came from the Inter-University Consortium for Political Research archive;

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omission of the Oregon Republican gubernatorial vote for 1906 is probably the most serious example. Unusual figures should be checked against other sources. Second, statewide percentages represent a net effect of turnout and preference over national, state, and local levels. One often cannot discern from state data alone important changes at local levels, such as the Democratic swing of urban areas in 1928, and one may mistake national or regional trends for state trends.

The party strength indexes deserve even more cautious use, since they are averages of several races—sometimes from different years. The 1916 California Republican index "A," for example, combines percentages for the Progressive Republican Johnson, the regular Hughes, and a variety of congressional candidates, plus gubernatorial percentages from 1914 and 1918—the former from a regular who had been defeated by Johnson running as an independent Progressive. The indexes also muddle important electoral discontinuities because averaging and extrapolation tend to smooth data as well as to give deviant cases, such as 100 per cent values from uncontested elections, disproportionate weight.

Finally, it is disappointing that this book offers no better analytic models than the familiar examination of predefined regions and time periods. Classification of states by average party strength, 1896–1930, distorts the picture for states that had rather different periods of relative electoral stability, and it ignores variance as an indicator of competition. Thus a volatile Idaho is classified "predominantly Republican," and a stable Delaware "competitive" for 1896–1930 because, though Delaware Republicans won much more consistently, they did so by narrower margins. Political historians can gain greater insights from electoral data by using inductive methods to differentiate state and nation-wide effects for each year, to discover individual state trends, and to reveal groups of states that behaved similarly in different time periods.

In any case, the foregoing critical discussion should not overshadow the fact that Professor David's careful work gives students of American politics the sort of concise, easy-to-use historical election statistics they have long needed. His book will undoubtedly become a standard reference tool and starting point for future electoral research.

JOHN L. MCCARTHY
Yale University

H. DUANE HAMPTON. *How the U.S. Cavalry Saved Our National Parks.* Bloomington: Indiana University Press. 1971. Pp. 246. $8.95.

By 1886 Congress and the Department of the Interior were in their fourteenth year of bickering over how to run the Yellowstone Park. The army in that year was given the task, and it policed the reservation so well that Yosemite, Sequoia, and General Grant National Parks subsequently fell under cavalry control. In all of these places poaching, illegal grazing, and spoliation were curtailed or entirely ended. During the same era pertinent new laws were passed and policies evolved, some of which were continued when the National Park Service took over in 1916.

This so-called army period is a phase of national park history that needed to be told, and Professor Hampton has done a solid, commendable job. In the balance the army carried out its assignment well, and this judgment is clearly set forth and admirably substantiated in this book.

Therefore it is unfortunate that the study is weakened by the author's crusading position that the cavalry "saved" the national parks. Semantics aside, most historians who have studied national park history will question this stand. To assume that the powers of darkness would have succeeded after 1886 in destroying the national parks, had the army not stepped in, is to ignore the zeal and intellect of such individuals as George Bird Grinnell, Arnold Hague, John F. Lacey, John Muir, and Theodore Roosevelt. It is to omit consideration of the political power of the Boone and Crockett Club and the Sierra Club and the propaganda dispensed by such journals as *Forest and Stream* and *Century Magazine.*

Moreover, in his crusade for the cavalry the author is guilty of omissions on the negative side. The army record was good, but it was not above criticism. The military tended to collaborate with the concessionaires, and it was dis-

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tinctly class-conscious. It often resisted innovation. The rapid turnover of personnel was detrimental to good park administration. Little if anything is said of these and other army weaknesses.

Thus this book, which is still valuable, well researched, well organized, and well written, is damaged by being a polemic for the United States Cavalry. It need not have been.

Several fine illustrations and a map add to the merits of the book.

RICHARD A. BARTLETT
Florida State University

ARTHUR S. LINK *et al.*, editors. *The Papers of Woodrow Wilson*. Volume 10, *1896–1898*; volume 11, *1898–1900*. (Sponsored by the Woodrow Wilson Foundation and Princeton University.) Princeton: Princeton University Press. 1971. Pp. xiv, 610; xv, 607. $16.00 each.

These two volumes in this splendidly edited series carry Wilson through summer 1900. As in the earlier volumes, the editors have included Wilson's course outlines, notes for class lectures, published essays, articles, lectures, and orations as well as newspaper reports on his public addresses. In great demand as a lecturer and writer, Wilson was able to supplement his professor's income with fees and royalties; but it was clearly not for that alone that Wilson gave so much to his writing. In one letter he confesses to a yearning for a "literary career," and elsewhere remarks that "there ought to be a genuinely first class narrative in me somewhere."

Oddly, it was in just these years that the quality of his literary work dropped sharply. His style became self-consciously elegant and wordy, and the pieces on Burke, Bagehot, and Maine seem more nearly orations than lectures polished up for publication. Conscious of a certain artificiality in his writing, Wilson once explained, "I write in sentences rather than in words: they are formed whole in my mind before they begin to be put upon the paper, usually—and no doubt that is the reason they seem *cast*, rather than naturally poured forth." On writing history, he advised, "You must produce in color, with the touch of imagination which lifts what you write away from the dull level of mere exposition. Black and white sketches may serve some purpose of the artist, but very little of actual nature is in mere black and white." There, observed a critic, was the rub. Whether because he was so bent on "producing in color" or because of the distraction of his many projects, the quality of Wilson's historical work suffered during these years. *George Washington* was probably his weakest book, and a "colorful" but vapid comment on a paper presented by Frederick Jackson Turner to the AHA seems almost to have been tossed off with the left hand. Yet in his writings in these volumes and especially in his course outlines and lecture notes, one can trace the continuing development of Wilson's ideas on government. Of interest was his increasing awareness of the need for universities to train a new generation of experts for the public service—"teaching them the science of administration."

A good part of these volumes deals with Wilson the college teacher and academic citizen at Princeton. Regarded by colleagues and trustees as a faculty leader, the strong-willed Wilson almost inevitably had become a rival of President Patton, and their mutual dislike and resentment were barely concealed. Points divided roughly evenly. When the University of Virginia offered its presidency to Wilson—only the most tempting of a number of college presidencies proferred—several trustees countered with a private purse that handsomely augmented Wilson's salary for the next five years. Patton was informed rather than consulted. On the other hand, Wilson, for all his standing, was not able to bring about the appointment to the Princeton faculty he most dearly wanted; Frederick Jackson Turner's Unitarianism, Patton and several trustees decided, disqualified him for a post at Princeton. Score one for Patton.

During his lecture tours and travels, Wilson wrote almost daily to his wife, and these letters are among the most delightful items in the volumes. With eloquence Wilson writes of his deep love for Ellen, and his dependence on her, and in one revealing passage acknowledges even more: "the supreme part, that love plays and must ever play in my life—my capacity for loving, my need for being loved,—the almost feminine sensibility I have with regard to the feeling others may have for me,—an insatiable desire to be loved."

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The volumes are revealing not only of Wilson but of the middle-class academic world he lived in—a world where universities were for gentlemen, where breeding and family background counted, probably more than scholarship (Wilson never lost an opportunity to identify himself as Scotch-Irish, a Southerner, and a Virginian), and where Turner could be rejected as the wrong kind of Protestant. Wilson fought that, but with most else he was comfortable. He accepted as given the superiority of the Anglo-Saxon race and could seriously explain the contribution to it of various blood lines: "the things that make us steady in government we get from the Normans."

Nothing need be added here to the praise reviewers of earlier volumes have heaped upon the editor. It is all deserved.

HERBERT J. BASS
Temple University

DANIEL LEVINE. *Jane Addams and the Liberal Tradition*. Madison: State Historical Society of Wisconsin. 1971. Pp. xviii, 277. $8.50.

The central focus of this work is upon Jane Addams's role in American reform thought. Attacking both the consensus school and the New Left interpretations, Daniel Levine sees Jane Addams as a radical within the American liberal tradition. This is largely accomplished by defining radicalism as a "stance towards the political process, not any particular program" (p. xv). By wishing to change society more rapidly than most of her contemporaries, Jane Addams thus becomes a radical particularly in respect to her crucial beliefs in environmentalism, the virtuousness of mankind, and the ideal of community over the individualistic ethic. The latter two are most radical since they represent a major thrust against the rational self-interest of Lockean liberalism, the taproot of the American liberal tradition.

Yet there are curious contradictions in the book. Jane Addams is characterized as weak and ineffective as a politician and most effective as a publicist, but her activities as a publicist take second place to an analysis of her ideas, none of which is claimed to be original. Her work to moderate disputes in many fields and her compromising temperament are caught, but they appear to be far less important to the author than her ideas. Part of this fault may lie in the book's format where the major theoretical emphasis is placed in the introduction and the afterword with a three-section biographical essay sandwiched in between, much of which is well known. Even in her pacifism Jane Addams is characterized as being unable to stand outside society as a rebel; perhaps she needed the adulation to which she had become accustomed, although Levine proclaims that the admiration she received obscured her importance.

Important contributions to understanding Jane Addams and the progressive reformers are made in the discussions of her relationship to coworkers at Hull House, her stint on the Chicago Board of Education, her ability to finance Hull House, and, especially, her capacity to learn from experience, which constitutes a direct assault upon the psychohistory school that tends to set Jane Addams as an inhibited Victorian compensating with a strong sense of *noblesse oblige* through her social welfare activities. Yet there is not an effective analysis of how the role of womanhood affected Jane Addams although she always viewed prostitutes as being victims of "evil men." There is also a danger in the central thesis of forcing all varieties of reform thought into a liberal tradition that seems to be vague enough to become a new liberal consensus theory. If, however, this book causes scholars to reflect upon their own categorical assumptions about reform and reformers as it might, then it will certainly deserve that old cliché—"a welcome addition to the field."

LOUIS L. ATHEY
Franklin and Marshall College

WILLIAM REYNOLDS BRAISTED. *The United States Navy in the Pacific, 1909–1922*. Austin: University of Texas Press. 1971. Pp. xii, 741. $15.00.

In the research for this book, Professor Braisted has consulted every source of significance in both the United States and Japan, though evidently his research in British sources was not so thorough. Private papers, government documents published and unpublished, memoirs, and secondary materials were all consulted, as a twenty-two page bibliography clearly testifies. The result is a massive study,

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meticulously written, of American naval policy pertaining primarily to the Pacific during the period 1909 to 1922.

It is not, however, a study of American naval operations in the Pacific. Professor Braisted says very little about the way the navy was organized for Pacific operations or the way in which operations were conducted and administered. One must not conclude, however, that it is not a good study. The main problem seems to be that the book is mistitled. It is really a history of American naval policy pertaining mainly to the Pacific but also to Atlantic policy vis-à-vis Great Britain.

As a study of American naval policy the book is superb. It shows clearly that whatever one may say about a military-industrial complex dominating American military and foreign policies in the post–World War II era, naval policy in the early twentieth century was determined much more by nonmilitary considerations. The climactic event of the period, the Washington Conference, illustrates this point very well. The Five Power Treaty on naval limitation came about in large part because Secretary of State Hughes and the civilian secretaries in the Navy Department recognized that the navy "second to none" envisioned in the naval policies established at the time would not be built because of various pressures by Congress and public opinion. Professor Braisted also shows that naval officers were not of one mind about what kind of policy ought to be followed in the Pacific, so that statements of "official" policy by agencies such as the General Board were frequently undermined by contrary statements made by other agencies or by individual high-ranking officers.

The book is exhaustive in its detail in some areas but seems superficial in others. For example, there is a long chapter on the problem of building a dry dock at Pearl Harbor and another on the behind-the-scenes efforts to secure commitments from the Chinese government to build several warships in American yards; on the other hand, there is no mention of the effect of the Mexican crises of 1914 upon naval planning for the Pacific. Also, the chapters on the Washington Conference are comprehensive in what they cover, but there is no discussion of the supplementary treaty on submarine warfare although it is mentioned as being one of the results of the conference.

These criticisms, however, do not detract from the overall value of the book. It is the only work of significance on American naval policy in this period, and, along with his earlier book covering the period 1897 to 1909, it marks Professor Braisted as the authority on the subject. This book will undoubtedly remain the most authoritative work for many years on the subject of American naval policy in the Pacific prior to 1922.

ERNEST ANDRADE, JR.
University of Colorado

DAVID MORGAN. *Suffragists and Democrats: The Politics of Woman Suffrage in America.* [East Lansing:] Michigan State University Press. 1972. Pp. 225. $7.50.

An analysis of the culminating phase of the long campaign for woman suffrage, by a British political scientist, creates an agreeable expectation that a subject that has awaited objective treatment might at last have received its due—an expectation this book does not wholly satisfy. The author states his intention to deal with the "practical politics" of the passage and ratification of the Nineteenth Amendment once it had reached the arena of national politics "after 1912," focusing "on the period marked out by Wilson's campaign for re-election in 1916 down to the ratification" in August 1920. He does not deal with the role played by the suffrage issue between 1910 and the disruptive election of 1912 that put the Democrats in the saddle; and only cursorily with the slow development of Wilson's perception between 1913 and the fall of 1916 that the suffrage tide was irreversible; and that his party must deal with the issue or be overwhelmed by it.

The study's most valuable contribution (chs. 7–11) is its analysis of the vexing problems created for the Wilson administration by intraparty conflict between the Southern congressional bloc, implacably hostile to political equality, and the Democrats from west of the Mississippi, some of whom were accountable to women constituents after 1912. The dilemma was made more acute by the fact that the suffrage issue was entangled with the concurrent

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issues of prohibition and child labor legislation. This unfortunate circumstance made strange bedfellows.

The author has foraged widely among suffrage papers and among the published and unpublished papers of Woodrow Wilson, John Sharp Williams and other congressional leaders (mainly Southern), and several prosuffrage members of Wilson's cabinet including McAdoo, Daniels, Burleson, and Lane. The voice of John Sharp Williams comes through most clearly in the dialogue between opponents and advocates, expressing the Southern politicians' genuine fear of what the ballot in the hands of black women might do to the shaky foundations of a white supremacist society. Neither Wilson's earnest pleas nor threat of consequences won any converts among this group.

Despite the author's initial statement regarding the scope of his study, he devotes half of this relatively short book to a review of the legal, economic, social, and educational changes affecting women from the mid–nineteenth century onward and runs the risks common to those who attempt to summarize such a mass of material. While Morgan's generalizations are often provocative, the regional diversity and uneven rate of change in American social history reflected in the suffrage movement continually elude him. Therefore, the validity of many inferences regarding motivations and attitudes of suffrage leaders is flawed by errors of interpretation or weakened by inexplicable omissions. The reader with more than a casual knowledge of women's history is frequently disconcerted by errors in dates and names: for example, it was Lucretia Mott and not Elizabeth Cady Stanton who was an accredited delegate to the World Anti-Slavery Convention in 1840 and stirred up the debate that launched the feminist movement. In this connection more rigorous editorial supervision would have been helpful.

Dr. Morgan is correct in saying that the checkered history of the suffrage movement offers an illuminating and often unflattering view of the actual workings of the American political process, and his analysis is best when he is using primary data to dissect that process at the level of national politics. My regret is that he did not devote more space to that part of his study. Included in the volume is a comprehensive bibliography.

LOUISE M. YOUNG
Washington, D.C.

JUNE SOCHEN. *The New Woman: Feminism in Greenwich Village, 1910–1920.* [Chicago:] Quadrangle Books. 1972. Pp. xi, 175. $6.95.

In the years after 1910, hundreds of young women went to Greenwich Village in self-proclaimed revolt against the traditional values of American life. They wanted to make careers for themselves in journalism, social work, and the arts. They dreamed of achieving political power and of utterly remaking American society. They were determined to be as sexually uninhibited as they believed men to be.

June Sochen has studied the general phenomenon of the "New Woman" through the lives of five representative figures: Crystal Eastman, Henrietta Rodman, Ida Rauh, Neith Boyce, and Susan Glaspell. But while the author gives us a good many interesting biographical facts about these women, she fails to ask any of the analytical questions that could have made her book an important contribution to American cultural history. Why is it, for example, that these particular women were radicalized? Was it some maladjustment in their home lives that was responsible?

Maladjustment, however, is not the sort of material in which Professor Sochen deals. For although she is admirably honest in showing us that most of the feminists' dreams ended in failure, she nevertheless conceives of her representative women as heroic pioneers whose aborted revolution has now been revived by Betty Friedan, Kate Millett, Shulamith Firestone, and other heroines. More of a myth-maker than a historian, Professor Sochen ignores the psychological problems of her subjects in order to create a "usable past" for the feminists of the 1970s.

KENNETH S. LYNN
Johns Hopkins University

JULIAN F. JAFFE. *Crusade against Radicalism: New York during the Red Scare, 1914–1924.* (National University Publications, Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1972. Pp. viii, 265. $12.50.

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New York State was second only to the federal government in Washington, D.C., in its attempts to suppress radicalism and subversion during and after the First World War. Mr. Jaffe's well-organized though limited study synthesizes much that was known about the state during the Red Scare period and adds detailed information about the politics of antiradicalism in New York City and in Albany. Read as narrative, it is the depressing record of the use of governmental authority and pressure against left-wing groups that the author aptly characterizes as posing little realistic threat to the nation's stability or direction.

Dealing chronologically and topically with prewar radicalism in New York, the wartime and 1919 Red Scares, the Red Scare in the schools, the infamous Lusk Committee, the unseating of the five socialist assemblymen in Albany, the Palmer raids, and the prosecution of criminal anarchy cases in the courts, Jaffe is best in recreating a sense of the creative ferment of radical politics in prewar New York City and in recording the efforts of politicians and patriots to crush radicalism in any form. His book is less helpful in explaining why the antiradical crusade occurred in New York or elsewhere and whether, indeed, it represented a popular crusade from below at all or a loosely coordinated suppression from above.

Drawing heavily on public documents, newspapers, and the papers of prominent officials, Mr. Jaffe's study concentrates almost exclusively on the actions and interactions of poitical leaders, governmental agencies, and organized pressure groups. Though the author credits the public with having influenced events, his evidence leans in a different direction. Causation would appear to be a fortuitous wartime and postwar combination of nativism, economic problems, antilabor agitation, anti-Bolshevik feeling, rural-urban conflicts in New York State, and a healthy dose of political opportunism. While no doubt accurate on one level of analysis, such findings tend to cloud rather than illuminate other possibly more significant factors.

Deep national disturbances such as a red scare are more than the sum of their visible parts. On their irrational side they are often marked by severe misperceptions of reality, by psychological projection, by the creation of vast conspiratorial myth systems that distort reality beyond recognition, shaping it to suit the psychic needs of a society under stress. In passing, Mr. Jaffe brushes against this side of the Red Scare, but his suggestion that his study substantiates the findings of scholars who have directly confronted the less rational side of this phenomenon is questionable at best. Indeed, his own attempt to justify admittedly irrational fears in 1919 on the basis of "the totalitarian nature of communism in later years" brings into question his own understanding of this side of the problem. Mr. Jaffe's book, however, is useful for its specific information on New York during a critical phase of the nation's past. It is unfortunate that the price set by the publishers for the book renders it almost prohibitively expensive for many who would be interested in the study on its merits.

LES K. ADLER
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D'ARCY MCNICKLE. *Indian Man: A Life of Oliver La Farge.* Bloomington: Indiana University Press. 1971. Pp. xiii, 242. $7.95.

The late Oliver La Farge was a true romantic. Enchanted with the exotic, he broke out of a genteel Eastern matrix to spend most of his adult years close to the Navajo Indians. His not inconsiderable ability and energy were diffused in a miscellany of occupations, in none of which he attained durable distinction. Starting out as a graduate student in the ethnology of the Southwestern Indians, he wandered into Mayan archeology. A disappointing interruption in his professional career afforded the occasion for the writing of a romantic novel, *Laughing Boy* (1929), whose instant success entailed other diversions from scholarship. For a decade he divided his time between imaginative writing, labors on behalf of Indian tribes, and archeology. Whatever his original promise, he found himself at forty without a profession and dependent for support on hack writing. World War II rescued La Farge, as it did many others, from a life of desperation and enabled him to indulge his romanticism for a few years as a military historian traveling to exotic places under exciting, enjoyable, and remunerative circumstances. Those seem to have been

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the best years of his life; after the war he devoted himself almost fully and fruitlessly to the cause of the Indians. Characteristically, he permitted himself then, as earlier, both extravagant expectations from each new administration in Washington and disappointment with political reality.

The portrait that emerges from La Farge's autobiographical *Raw Material* (1945) is confirmed by this informed and compassionate biography. McNickle recognizes the many excellences of La Farge and, it seems to me, discerns the element that supported him in an endless sequence of frustrations and sorrows but ultimately left him a pained and pathetic caricature of the promising young man. La Farge was an incurable optimist whose career presents the perfect embodiment of the romantic mind somewhere described by Santayana.

THOMAS LEDUC
Oberlin College

JOACHIM RADKAU. *Die deutsche Emigration in den USA: Ihr Einfluss auf die amerikanische Europapolitik 1933–1945*. (Studien zur modernen Geschichte, Number 2.) [Düsseldorf:] Bertelsmann Universitätsverlag. 1971. Pp. 378. DM 38.

Here is a study of the politics of the German emigration to the United States, not a run down of who arrived and where they went but the political ideas and activities of the refugees. The author shows that many of the Germans found the New Deal era sympathetic and were able to see in the liberal outlook and actions of Franklin D. Roosevelt's administration the sort of approach to the Great Depression that might have worked in Germany, had not the greatest adventurer of all time seized control of that unhappy land. To be sure, there were all shades of refugee opinion, from communism to a staunch conservatism, and there were awkwardnesses as well as fairly easy adjustments. By and large the opponents to Hitler found American politics congenial.

The influence of the refugees upon American political life is much more difficult to show, and the author seems unsure of what it amounted to. That the new Germans in America possessed many ideas is beyond dispute, but the effect of those ideas on American politics is hard to measure. In the initial period of fitting themselves into American life the refugees were necessarily tentative and awkward, and their major influence probably came after 1945. Careers moved forward slowly. By the time of his death Hajo Holborn was the veritable *doyen* of German historical studies in the United States, but his better-known students and the bulk of his historical writing dated from the postwar years. Holborn found it intensely difficult to write in English until he had almost twenty years of practice. Adjustments were not easy. Some of the refugees found political life almost impossible in the New World. Heinrich Brüning, because of his eminence in the Weimar era, could not easily participate in another system. Thomas Mann was stiff-necked and awkward in offering political opinions. The huge Jewish emigration from its centers in New York and Hollywood sensed its differences with the bulk of American Jewry, which was from Eastern Europe, not Germany.

The author shows an enviable mastery of his complex materials and confirms his findings in 1,889 notes, numbered consecutively throughout the text. Only once did he falter in identification of American political figures, as when (p. 63) he confused Francis E. Townsend with Peter Townsend.

ROBERT H. FERRELL
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JOHN P. DIGGINS. *Mussolini and Fascism: The View from America*. Princeton: Princeton University Press. 1972. Pp. xx, 524. $16.50.

This is didactic history. We are to learn lessons from "America's admiration of Mussolini's Italy" that "may help us analyze the fascism within ourselves." The main subject of this work is U.S. self-delusion until the mid-1930s about the nature of Fascist Italy. Frequently the praise was more for the Duce, the "human pseudo-event," than for his eclectic philosophy, and many American nativists were pleased to imagine that fascism meant the "Americanization of Italy." Although almost half the book is devoted to the period after the Ethiopian war of 1935–36, from this date on Americans lost any clear image of Mussolini's regime, lumping Italy's new "war fascism" with nazism,

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while the direct involvement of the U.S. in Italian affairs between 1943 and 1945 has been told before.

Diggins's research is exceedingly thorough and far-ranging. Unfortunately he has not resisted the temptation to include far too many tidbits, and his book is somewhat wordy. He is skillful in distinguishing among American societal forces, but it is tedious to read again and again of successive groups' naive acceptance of Mussolini at his anti-Bolshevik face value and of his corporative state as a revolution. And is it really necessary to discuss John Dewey and Hannah Arendt, who, by the author's own admission, had little to say about Italian fascism? The density of the material seems to affect the organization, which often willfully flouts chronology. The book is written in lively enough style, albeit forced in places. Thus we go from felicities ("for most Catholics Fascism was not a matter of truth but of consequences") to gaucheries ("the Duce was obviously trying to bury the hatchet of sanctions").

Diggins is more at home in American than in European history where he is guilty of some loose generalizations. For instance, Dino Grandi's reputation was more (or less) than that of an "urbane diplomat," which was why the U.S. anti-Fascists demonstrated against his visit in 1931; it is a little unfair to expect many interwar Americans to be able to understand nazism by reading *Mein Kampf* when Hitler refused to authorize an English translation; and one really cannot write: "when Hitler invaded France in September 1939."

Mussolini and Fascism is most successful and useful, then, in establishing once and for all the broad U.S. welcome afforded early fascism. It speaks volumes about the temper of American society in the twenties, and the point that the spell of fascism operated beyond Europe is well taken. On the other hand, the relevance to present discontents will appeal to some readers more than others.

ALAN CASSELS
McMaster University

GEORGE CARROLL DYER. *The Amphibians Came to Conquer: The Story of Admiral Richmond Kelly Turner.* In two volumes. With an introduction by ERNEST MCNEILL ELDER. [Washington: Government Printing Office. 1972.] Pp. xxv, 596; ix, 597–1278. $14.50 the set.

Successful military commanders fit no set personality pattern. Some are high strung, others phlegmatic; some are gracious and considerate of their subordinates, others demand a high performance that is second only to the requirements they place on themselves. Admiral Richmond Kelly Turner was one of the latter. A man of towering intellectual capacity and drive whose nickname was "Terrible," he was the Navy's chief planner from 1940 to 1942 and then the pre-eminent leader of the amphibious forces that inexorably drove Japan back from Guadalcanal to her home islands.

In Vice-Admiral George C. Dyer, Turner has found a friendly but not awestruck biographer. Dyer has approached his project with the energy and determination of a top-flight staff officer assigned to prepare an after-action critique and narrative. If Dyer has missed any facts about Turner's life and career, I will be surprised. The result is a long study that includes an outstanding account of the amphibious aspects of the war in the Pacific; an impressive study of the war preparations before Pearl Harbor; a long-needed, pro-Turner account of his part in the efforts to read Japanese intentions immediately before that attack; and with it all a good social history of the Navy between 1908 and 1947.

Despite all of its good points, *The Amphibians Came to Conquer* will be read in its entirety by only a handful of hardy naval historians. It is too long, too detailed, too quotation filled, and too encyclopedic to appeal to most readers. That readership could have been enlarged had the book gone through more drastic editing; but that would have cost much of its appeal and completeness. Nevertheless, it is a book that must be used by any student of naval operations in World War II and will be ignored at considerable peril by those concerned with the history of the prewar Navy. The two volumes are well supplied with photographs and excellent maps. Admiral Dyer, in short, has produced a major contribution to the historiography of World War II but one whose concept and execution will inhibit its use.

K. JACK BAUER
Rensselaer Polytechnic Institute

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THEON WRIGHT. *The Disenchanted Isles: The Story of the Second Revolution in Hawaii.* New York: Dial Press. 1972. Pp. xiv, 304. $7.95.

The Disenchanted Isles deals with the transition of the "Last of the Magic Isles" from a mid-Pacific paradise to a modern American industrial community and an aggressive state. This revolution was quite unlike and totally unrelated to that of 1893, fomented largely by American sugar and commercial interests and dominated by "Mission Boys"—sons of early missionaries—who forced Queen Liliuokalani's abdication and schemed for annexation to the United States.

A missionary-sugar complex practically controlled, both politically and economically, the Hawaiian Republic, an oligarchy, and the undemocratic territory for a half century. This was the period of the domination of the Big Five-GOP coalition when few non-*haoles* and almost no Orientals ever walked into the Pacific Club, which was the social shrine of *kamaaina haoles.*

The second revolution has taken place gradually during the last thirty years and especially since World War II. That upheaval unleashed new political forces, which sparked a quiet rebellion of Hawaii's second-class citizens. These included Portuguese, Japanese, Chinese, Filipinos, and even Hawaiians led by Jack Burns, a former police chief who rose to become delegate to Congress and who as governor guided the statehood bill to passage, and by Daniel Ken Inouye who, after sacrificing his right arm in heroic action in World War II, was denied a haircut in San Francisco. Statehood shifted the focus of political control from the *haole* elite to the former second-class citizens, with the second revolution sending to Washington Hiram Fong, the first of oriental extraction to sit in the United States Senate. He was later joined by Daniel Inouye, along with Representative Patsy Takemoto Mink.

Hawaii's closed economy shifted to an open economy, with the "Chinese tycoons" among the salient factors in the islands' transition to statehood. Hung Wo Ching, after making Aloha Airlines pay, became a director of the Bank of Hawaii; his brother, Hung Wai, created a financial empire that ultimately placed him on the Board of Regents of the University of Hawaii; while Hiram Fong grew wealthy in the real estate and insurance business. Sugar was no longer king. Expenditure on defense, tourism, and merchandising equaled or exceeded income from sugar. A thirty-year plan for the islands, known as Hawaii 2000, is now underway.

Theon Wright, a successful journalist turned writer, has succeeded in presenting a fascinating 293-page treatment of the "Disenchanted Isles" with only forty-four notes, though he seems oblivious to the inaccuracy of the commercial index.

MERZE TATE
Howard University

LATIN AMERICA

HORACIO LÓPEZ GUÉDEZ. *Los reyes católicos y América (1492–1517).* Mérida, Venezuela: Universidad de los Andes, Facultad de Humanidades y Educación. 1971. Pp. 74.

The contents of this slim volume are not accurately reflected by its title. Not one of the three sections really focuses on the apparent subject of the volume. The author is more concerned with what structure the Spanish state had by 1492 and its response to America during the Habsburg years (1517–1700) than in the years between 1492 and 1517.

The book opens with an examination of the background to the Columbus voyages—the role of the Church, the "Alexandrine" bulls, and the precedent of the Portuguese bulls—as a function of preparation for exploration of unknown and un-Christian lands. It is followed by discussion of the incorporation of America into the monarchy of Castile-León and the origins of administration in Spanish America, listing the offices and titles granted to Columbus and their origins in Spanish administration and noting the conflicts inherent in the varying interests of state, Church, conquistadores, Spaniards, and Indians. The concluding section discusses the laws of Burgos (1512) and Valladolid (1513) in the context of the political objectives of the Catholic monarchs in America.

Although the volume is well documented and has an extensive bibliography, it adds little if anything to existing knowledge. It is useful only as a short, summary work on the problems faced by the Spanish state in its first quarter century in the New World, with references for further reading. Students in particu-

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lar should be wary of the author's obvious anti-Spanish, anti-Catholic bias and his occasional unorthodox use of traditional sources.

BRUCE B. SOLNICK
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GLEN BARCLAY. *Struggle for a Continent: The Diplomatic History of South America, 1919–1945*. New York: New York University Press. 1972. Pp. 213. $7.50.

It is a great pity that diplomatic historians and specialists in international politics have very largely ignored the fascinating body of historical evidence contained in the record of the intraregional and extraregional political and economic relations of the Latin American states. The pattern of these relations represents a microcosm of international relations in general, and the analysis of it can lead to fascinating hypotheses, models, and theories of more general application. It is, therefore, all the more unfortunate that Glen Barclay's book *Struggle for a Continent: The Diplomatic History of South America, 1919–1945* is such a stunning disappointment. Barclay presents an interesting thesis—that a weak country can defy a great power that wishes to dominate if the weak country possesses sufficient gall, diplomatic skill, and luck—and the case study he uses to prove the thesis, relations between Argentina and the United States between 1919 and 1945, is a very interesting one. Unfortunately, he does it very badly. This is a great pity, but it is an indication of the dearth of materials in this field. Even so, I must reluctantly recommend the book to those interested in the subject matter.

It is unnecessary to give an extended précis of the contents of the work. Basically it traces the story of how successive Argentine governments, between 1919 and 1945, managed to increase the wealth and power of Argentina at the expense of the political and economic misfortunes being suffered by the rest of the world at large, while outmaneuvering the United States, Brazil, and both the World War II Allies and the Axis powers. By the middle of 1945 Argentina emerged as by far the wealthiest and most powerful country in Latin America and a member in good standing of the United Nations, despite having traded with Germany practically up to the moment Hitler was immolating himself in his Berlin bunker. While all this was going on one Argentine foreign minister had even managed to get himself awarded the Nobel Peace Prize.

It is a fascinating and instructive story. My objections to Mr. Barclay's treatment of it fall into three categories. First, he has made use of a limited set of sources (*La Prensa* of Buenos Aires is the principal one), and one gets the impression that no field work was performed. In addition, some of the references are presented in such a way as to make follow-up difficult or impossible.

Second, the book is loaded with misstatements, hyperbole, and biased terminology. Only a few of the very worst examples can be cited here. We are informed that as of 1919 the ABC powers "represented . . . literally the only group of historically mature, constitutionally stable, traditionally peaceful . . . states to be found anywhere in the world" (p. 27). He appears to be unaware that oil had anything to do with the Chaco War (p. 43). We are told that Argentina and Brazil have always been at peace (p. 26). As of 1930 Colombia, according to Barclay, had no political parties (p. 62). Several Latin American regimes as of 1940 were "notably less beneficent, liberal, or humanitarian" than the Nazi government (p. 66). In the 1940s Brazil, it seems, had designs on Dutch New Guinea (*sic*) (p. 112). The Caribbean countries are "banana belt vassals." In fact, there is an anti-Latin bias quite evident throughout the book ("There was no such correlation between the promises and the real intentions of any [*sic*] Latin Government" [p. 203]).

Finally, there is an appalling breakdown in proofreading. As many Spanish and Portuguese words are misspelled as are spelled correctly (*La Prensa* is *La Presna* throughout). Either the author has no acquaintance with these languages or he never read proof.

Despite all this, as previously stated, the book inadequately fills a gap where there is precious little else. Let me finish with a plea to more competent scholars to consider devoting at least some of their talents to studies of the diplomatic history of Latin America.

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P. J. BAKEWELL. *Silver Mining and Society in Colonial Mexico: Zacatecas, 1546–1700.* (Cambridge Latin American Studies 15.) New York: Cambridge University Press. 1971. Pp. xiii, 294. $13.50.

History is not the collectivity of either biographies or local histories; yet "history" needs to be checked constantly against the lives of the men of the time and the chronology and detail of local events and customs. At a time when global theories explaining and interpreting the significance of the colonial status of Latin America and the problems of interracial contacts are so popular, Latin Americanists should be more than ever cognizant of the underdeveloped state of the study of their area. More microeconomic and local social history must be written for us to test the new broad interpretations.

P. J. Bakewell has undertaken the task of writing the early history of Zacatecas, the great Mexican silver-mining center. This study, of necessity, involves an exposition and analysis of many themes: New Spain's northward thrust, the erection of the mining center as a city, the interaction of economics and politics on both the local and national levels, the economic structure of this mining center and its place in the life of New Spain, problems of the city's unusual urban logistics, questions of land ownership and property rights and transfers, the techniques of the silver-mining industry of the sixteenth and seventeenth centuries, labor supply, finances, mining law, taxes, the mercury monopoly, and the causes of the fluctuations in Zacatecas's silver output. The number of topics covered in Bakewell's wide-ranging research is most formidable and poses a problem in organization that was not completely solved. But the rhetoric is smooth, and the reader comes away from the book with a feeling of respect for the author's command of sources and the scope of his scholarship.

A most interesting aspect of Bakewell's discussion of Zacatecas's institutions and problems, based on his research, is his setting them against studies by other historians of other places in the empire or of the empire as a whole. For example, in discussing the corregidor, the *cabildo,* and relations between the *audiencia* of Nueva Galicia and Mexico City, he comments upon his findings—concerning the power of the corregidor, the composition of the *cabildo* and the influence of the miners in that body, its actual powers, and the effects of the sale of offices—by continually comparing them with the findings of Fr. Constantino Bayle, John Preston Moore, J. H. Parry, and Frederick Pike. The same is true of virtually every theme touched upon in the book. In some cases little new is added to our knowledge; in other cases the Zacatecas material challenges widely held beliefs—the Royal Fifth was usually the Royal Tenth—or suggests the necessity for further investigation in other regions—the reasons for and effect of mine owners investing in haciendas, the existence of a large mobile Indian proletariat and many black slaves, and evidence of the appearance of a native Mexican finance capitalism to supplant Crown activities during Mexico's splendid Baroque Age.

Most important, however, is the book's last chapter. Here Bakewell severely questions Woodrow Borah's concept of New Spain's "century of depression"; a number of Huguette and Pierre Chaunu's conclusions; and, also, by implication, the abject colonial status ascribed by Stanley Stein to Mexico vis-à-vis Spain and Europe. While population did decline, Borah's dates and evidence of depression are contradicted by the period of Zacatecas's prosperity and its labor history. The restructuring of New Spain's economy on a capitalist base as evidenced in Zacatecas may well have offset the effect of population decline expressed in raw figures. Indirectly, Bakewell sustains Bailey Diffie's thesis of vigorous manufacturing, especially of cloth, in the colonies and of the growth of an autonomous economy that would depress the importance of international trade. In short, in addition to a good job of writing local history, Bakewell has used his Zacatecas material to ask pertinent questions including whether Borah's "century of depression" really existed and whether Spanish mercantile policy warped the economy of New Spain as much as the conventional wisdom believes it did.

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PEDRO ALONSO O'CROULEY. *A Description of the Kingdom of New Spain, 1774.* Translated and edited by SEÁN GALVIN. [San Francisco:] John Howell—Books. 1972. Pp. xviii, 148. $10.00.

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This book is the first published version of a manuscript entitled *Idea compendiosa del Reyno de Nueva España* that reposed in the Biblioteca Nacional in Madrid for many years. Its author, Pedro Alonso O'Crouley y O'Donnell, was a Cadiz merchant who made several business trips to New Spain between 1764 and 1774. O'Crouley's interests, however, were not limited to commercial gain. He was an antiquarian, an art collector, and a member of several learned societies.

O'Crouley's work includes background summaries of pre-Hispanic Mexican history and culture; an account of the Conquest; and, for the time of his visits, data on numbers and ethnic distribution of the population, descriptions of flora and fauna and of ports, presidios, cities, and provinces, and an overall view of the condition of the kingdom. Figures on the latitude, longitude, and population of towns and cities, chronologies of viceroys and archbishops of Mexico, and lists of Indian tribes appear as appendixes. The author illustrated his *Description* with plans of forts and towns, sketches of native plants and animals, and a well-known set of drawings of race mixtures that were copied by contemporaries and subsequently circulated widely.

O'Crouley drew his information from well-known histories, official reports, knowledgeable persons he met, and personal observations. He used his sources critically and attempted to reconcile discrepancies. In content and style the work is more akin to the *descripciones* and *relaciones* of previous centuries than to the *Political Essay* of Humboldt, who made his observations only some twenty-five years later. But after all, O'Crouley, although a man of considerable intellectual sophistication, was not a trained scientist.

The translation, as far as I can determine without having access to the manuscript, is well done and the editorial comments pertinent and informative. The printing of the text and the duplication of illustrations are elegant. Mr. Galvin's book is clearly a labor of love, perhaps because of the affinity among the Irish that transcends time and space.

L. N. MCALISTER
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MICHAEL C. MEYER. *Huerta: A Political Portrait.* Lincoln: University of Nebraska Press. 1972. Pp. xvi, 272. $9.50.

Michael Meyer's "political portrait" of Victoriano Huerta fills a void in the historiography of the Mexican Revolution. The earlier period, that of Francisco I. Madero, has been well covered by historians in this country and in Mexico. The succeeding years, after Huerta's downfall in 1914, receive due attention in studies on Zapata, Orozco, the Convention of Aguascalientes, and Carranza's Constitutionalist era. William L. Sherman and Richard E. Greenleaf at Mexico City College tried to provide a new assessment of the Mexican warrior-president in 1960, but their research was confined to printed materials, and their conclusions added little that was new.

If objectivity is the ultimate aim of the historical method (though rarely achieved in practice) Meyer chose a difficult subject. It is probably impossible for a Mexican, even today, more than fifty years after the events, to give Huerta a fair evaluation. Emotions and prejudices still cloud the Mexican view of Huerta, as Meyer makes clear in his preface. When he looked through the records in the Foreign Relations Archives he found that a well-meaning, but scarcely impartial, anonymous clerk had written "The Ursurper" on Huerta's personal file. And a friend in Mexico asked Meyer how he could be fair to the general without denigrating Madero, the "Apostle" of the Revolution. Meyer gives us what we have long needed, a thoroughly dispassionate, historically sound account of Huerta's life and times. We are never likely to get closer to the truth of Madero's assassination than Meyer's analysis, which shows conclusively that Huerta, if he had some knowledge of the murder plans and did nothing to stop them, did not himself order the elimination of the deposed president and vice-president. In fact, exhaustive research is one of Meyer's strong points as a historian. He has left no stone unturned in discovering Huerta materials in Mexico, the United States, and Europe. (I might say, too, that this thoroughness is contagious. Meyer's students at the University of Nebraska are among the best-trained young historians in the Latin American field.)

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Meyer pulls no punches. Huerta intended to pacify Mexico, he says, come what may. The general attempted to restore peace by converting the country into "one huge military base" (p. 96). This was a military regime. The president even gave his civilian cabinet members the rank of general and asked them to wear uniforms at state occasions. But the revolutionary armies defeated Huerta's forces, and Meyer shows why. The federal soldiers were dragooned into the ranks, were untrained and unreliable, and deserted to the enemy at the first opportunity. The president did drink heavily, especially when his armies began to lose battles, though Meyer shows that, whatever detractors have said, he was never incapacitated by liquor. Meyer is at his best in describing the workings—or most often the nonworkings—of Huerta's regime.

The book is attractively printed, with footnotes at the bottom of the pages (where they belong) and with a pertinent line drawing heading each chapter. Meyer provides an excellent and well-annotated bibliography. It is altogether a most satisfying performance.

ROBERT E. QUIRK
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THOMAS P. ANDERSON. *Matanza: El Salvador's Communist Revolt of 1932*. Lincoln: University of Nebraska Press. 1971. Pp. ix, 175. $7.95.

Dr. Anderson has produced a workman-like treatment of an event of some historical significance, albeit on a limited scale. His study of a bloody episode, very important for El Salvador's subsequent development but lacking in broader ramifications, is essentially narrative and descriptive. While unpretentious and not particularly sophisticated, even a bit old-fashioned by contemporary standards, this modest volume neatly wraps up a neglected topic leaving a minimum of loose ends. Conscientious use has been made of the sparse published sources, supplemented by fairly heavy use of interviews. More inclined toward an intelligent and sympathetic appreciation of the Salvadorean experience than a searching and systematic analysis of root causes the author is quite successful in his re-creation of the state of affairs existing there some four decades ago. Lacking a comparative dimension to his work and hence somewhat parochial in his perspective Professor Anderson leaves the reader with the task of deciding what fundamentally differentiated this revolt from the general Latin American dissatisfaction with governments unable to cope successfully with the impact of the world depression. He does, however, furnish the requisite facts of the Salvadorean case, and this is itself a welcome contribution.

Anderson views the 1932 revolt as stemming from manifold reasons for peasant discontent augmented by the "economic disaster of the depression" (p. 21). He provides a detailed discussion of the revolutionary Left in that part of Central America and shows how the free and competitive election in 1931 of Arturo Araujo, an aristocratic liberal running on the Labor party ticket, unleashed expectations of change. The latter's replacement within nine months by an army general engendered a climate of disillusionment shading into dispair for radicalized sectors. Violence and widespread fraud during the municipal and congressional elections of January 1932 provided the detonator for a Communist-organized revolt. The chances of the national uprising for success were sharply reduced by the arrest in Guatemala of exile elements preparing to invade, the capture of the conspiracy's ranking leader four days before the revolt, and government disruption of plans for a coordinated mutiny within the army. Thus the revolt was limited to rural areas and quickly contained by the regime after a series of bloody episodes in provincial centers, each reconstructed in detail by the author. Minimizing the deaths for which the rebels could be held responsible, Anderson also opts for a far lower figure for victims of the government's repressive campaign than the conventional 25,000 or more. Citing "the physical problems involved in getting rid of such a large number of people in such a short time" (p. 135) he calls eight to ten thousand a more reasonable estimate. In light of events a century earlier in northern Brazil, where larger numbers were killed with more primitive military hardware, one could ask for stronger evidence than this sort of logic.

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H. A. WILL. *Constitutional Change in the British West Indies, 1880–1903: With Special Reference to Jamaica, British Guiana, and Trinidad.* New York: Oxford University Press. 1970. Pp. xvi, 331. $13.00.

The government of the British West Indies was subjected in the closing decades of the nineteenth century neither to severe challenge from below nor to sweeping change from above: both local pressures for reform and imperial interest in extending the crown colony form of rule were sufficiently spasmodic and tentative to ensure that the Caribbean possessions would remain inconspicuous during a period notable for political crises in other parts of the Empire. Developments in Jamaica, British Guiana, and Trinidad, the three colonies that this study treats in detail, were of an undramatic and limited nature, involving political changes on a scale insufficient to exert more than marginal influence upon established constitutional structures. Colonial secretaries found little time or enthusiasm for West Indian problems apart from Derby's modest concessions in Jamaica and Chamberlain's passing enthusiasm for prospects of gold in British Guiana before succumbing to the more substantial allure of the Witwatersrand. Policy remained predominantly the preserve of permanent officials and governors. An absence of personal papers restricts indiscretions to marginal comments on official files, though the dilemma of imperial representatives is clearly evident: power could not be entrusted to mainly white, and entirely selfish, oligarchies nor to spokesmen for the ignorant and illiterate majority. By the end of the century thoughts of constitutional progress had been superseded by doubts of the propriety of employing the elective principle in communities insufficiently British in racial composition. It was, however, too late for second thoughts: the ancient constitutions of the West Indies had resisted the introduction of the crown colony system long enough to allow the emergence of a nonwhite political consciousness that would now proceed to challenge both local oligarchies and imperial rule. Although Dr. Will frequently alludes to this development, one can only regret that his exhaustive account of constitutional developments is not accompanied by an equally authoritative analysis of social and economic changes that were preparing to disturb the apparent tranquillity of the British Empire in the Caribbean.

PETER MARSHALL
University of Manchester

JAMES MILLETTE. *The Genesis of Crown Colony Government: Trinidad, 1783–1810.* With a foreword by LEWIS E. BOBB. Curepe, Trinidad: Moko Enterprises. 1970. Pp. xviii, 295. $12.00.

Professor Millette's central thesis is that Trinidad as a newly acquired British colonial territory in 1797 had by 1810 pioneered a new system of crown colony government with direct imperial control from London that was to provide a prototype for later constitutional changes in the British West Indies. Jamaica as a result of its own constitutional crisis of 1838–39 (not to mention the repercussions this had on the main United Kingdom political parties of the day) and the Morant Bay affair of 1865, which led to the abolition of its and others' old representative system, has generally been regarded as the pace-setter for the other Caribbean territories—due in large measure to the memorandum prepared by Henry Taylor of the Colonial Office to argue against continued representative "democracy" in a multiracial society.

After surveying the final years of Spanish control, during which considerable progress was achieved under the enlightened administration of Governor Chacon, the author goes on to elaborate in considerable detail how the principal -and apparently most effective arguments against adopting British laws (and hence English representative institutions), as against the inherited Spanish codes, were those based on the idea that such a system was dangerous if not impossible to adopt in a multiracial society such as Trinidad. Not only were the usual ethnic/color divisions of a Caribbean colony present, but there was also the added problem of the Latin whites versus the expatriate Britishers and the political divisions (i.e. Jacobin versus Royalist colon) within the French emigré group. Most important of all, however, appears to be the fear on the part of the establishment (be they local oligarchy or colonial officials) that the more numerous mulatto residents would exploit their potential strength to become dominant.

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Yet there is little detailed information for all that on the status of the slaves and the free mulattoes or the latter's reaction to the political developments in the colony under the British regime. There is, however, considerable data covering the internecine strife between the French, Spanish, and English planters and merchants and those colonial officials who became involved. But this undoubtedly reflects the paucity of documentation concerning the nonwhite elements in general. Throughout the work there is constant reference to developments elsewhere in the British West Indies, especially Jamaica, so that a proper perspective for Trinidadian affairs in a Caribbean setting is provided for, thus avoiding the all too often narrow and particularized view of one Caribbean territory studied in a vacuum.

At certain times the detail is excessive, and the quotations are frequently too long. There are, too, the odd inconsistencies in the compilation of the bibliography. For example, *The History of the People of Trinidad and Tobago,* which was prepared for Trinidad's independence in 1962 by Premier Eric Williams, an Oxford-educated historian and for whom Millette is now a political opponent, is quoted in a footnote but omitted in the bibliography.

The study, however, has been thoroughly researched and is generally well written. It should provide us with a better understanding of the checkered beginnings of the crown colony system in the British Empire.

A. J. G. KNOX
University of Calgary

HÉLIO SILVA, with the collaboration of MARIA CECÍLIA RIBAS CARNEIRO. *1939: Véspera de guerra.* (Collection Documentos da historia contemporânea, Volume 11–J. O ciclo de Vargas, Volume 11.) [Rio de Janeiro:] Editóra Civilização Brasileira. 1972. Pp. xiv, 469.

According to the introduction, this eleventh member of the Ciclo de Vargas series is "the history of the preparation for the war." The book tells chiefly of United States-Brazilian relations and relies most heavily on the Vargas files and on the Department of State's volumes on *Foreign Relations of the United States* for 1939 and 1940. The translation of many documents contained in these volumes and of documents contained in the State Department's *Documents on German Foreign Policy, 1918–1945* will be useful to Brazilians unfamiliar with English.

The text consists of two sections. The first, "Unidade Continental," covers such matters as fifth-column activities in Brazil, Brazilian missions to the United States, the Panama Conference of 1939, the *Graf Spee* combat, Getulio Vargas's speech on board the *Minas Gerais* in 1940, the Havana Conference of 1940, Brazilian air bases, arms for Brazil, and tensions created when the *Siqueira Campos,* a Brazilian steamer, sought to defy the British blockade late in 1940.

For those who tire of the restatements of arguments (mostly taken from *Foreign Relations of the United States*) in the forty-one-page chapter on the *Siqueira Campos* affair, the book's second section, "Preparo para a Guerra," opens refreshingly with a condensed historical background of the Brazilian "iron question" from colonial times until 1939. Apparently much of this is based on an unpublished thesis, "Criação de Volta Redonda," by two Brazilian *professoras,* but a misplaced footnote makes it difficult to say how much. The account of negotiations in the United States in 1939 and 1940, which resulted in the construction of the Volta Redonda steelworks, contains many letters, especially from the Brazilian ambassador, Carlos Martins, and is drawn largely from the Vargas files.

Here again, in the case of iron and steel, it appears that this book is suitable mainly for Brazilians unfamiliar with English. Readers of English are fortunate to be able to turn to John D. Wirth, *The Politics of Brazilian Development, 1930–1954* (1970), and to Werner Baer, *The Development of the Brazilian Steel Industry* (1969).

Readers of *1939: Véspera de guerra* should not overlook the two chronological tables comprising the first ninety pages of the book. One deals with Brazilian iron and steel from 1554 to 1938, and the other concerns Brazilian and world events from 1938 to 1940. These numerous brief notes contain some interesting items about Brazilian matters not mentioned in the text.

JOHN W. F. DULLES
University of Texas, Austin

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibiliiy of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.

TO THE EDITOR:

Paul Gottfried's review of Julián Marías's *Generations: A Historical Method* (*AHR*, 77 [1972]: 730) shows no awareness of Marías's work and his standing in the Spanish intellectual world; no understanding of Ortega y Gasset; no familiarity with the literature on social generations; and, above all, no knowledge of the immediate context out of which Marías's book came. The reader of Gottfried's review would have no way of knowing that Marías wrote his book, which originated as a series of lectures, in response to Spanish critics of Ortega, chief among them Pedro Laín Entralgo. Nor would the reader suspect that Marías's volume contains the fullest bibliography on the social theory of generations to be found in English. Leaving aside the question of the validity of Ortega's theories and of his doctrine of human existence, it is beyond question that Ortega's writings, above all *En torno a Galileo* (1946–47) (and not *El tema de neustro tiempo* [1923] as Gottfried mistakenly asserts), contain the most elaborate and in many respects the most valuable discussion of the problem of generations to be found anywhere in twentieth-century contributions to the topic. In making the ludicrous suggestion that Marías's argument could have been reduced to a ten-page essay, Gottfried has simply revealed his own ignorance of a controversy in social theory that deserves to be much better known by American historians. Whatever its shortcomings—and it has many—Marías's book will contribute to the education of American historians; and for this reason its translation into English by the University of Alabama Press is to be welcomed.

ROBERT WOHL
University of California, Los Angeles

MR. GOTTFRIED REPLIES:

Dr. Wohl makes two valid points about my review that I readily concede, despite his ranting tone. Although I have read much of Ortega, I am not a specialist in Spanish intellectual history. His second defensible point is that Julián Marías, as Ortega's most famous *aficionado,* merited a fuller treatment in the pages of the *AHR* than he received at my hands. But then my responsibility was not to produce an essay on the glories of Spanish philosophy; it was to present my critical impressions in a very limited space of Harold C. Raley's translation of Marías's *Generations: A Historical Method.*

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My reading of the volume convinced me that its arguments were often tortuously stated and that its discussion of non-Spanish contributions to the generational problem did not always seem intellectually satisfying or sufficiently coherent. It should have been clear that I was examining the work as a book reviewer, not as a researcher preparing a monograph on twentieth-century Spanish thought. Viewed from this perspective there was nothing "ludicrous" about my remark that the book might have been greatly condensed. That was my considered response, somewhat qualified in the course of the review, to the rambling format of a particular text. It could not be considered as a judgment on Marías's importance as a philosopher.

I stand partially corrected on the matter of Ortega's "most valuable discussion" of the generation in history. Whereas Marías regards *El tema de nuestro tiempo* as a significant statement of Ortega's views on this subject, Marías, nonetheless, calls *En torno a Galileo* the more "mature" of the two works. I welcome Dr. Wohl's correction while deploring the utter gracelessness of his style. I might also have better appreciated the additional knowledge on the polemical background of the book in question if that knowledge had been offered as a point of information, not as an entirely dubious test of my professional competence.

PAUL GOTTFRIED
New York University

TO THE EDITOR:

Donald R. Kelley's provocative article "Martyrs, Myths, and the Massacre: The Background of St. Bartholomew" (*AHR*, 77 [1972]: 1323–42) exemplifies one of the dangers of a subjectivist and especially of a psychoanalytic interpretation of history; that is, the manner in which psychiatric categories may serve as unintended casuistry when they are used to classify human behavior without careful and constant reference to the external situation.

Kelley writes, after describing the persecution of French Protestants (p. 1330), that "it was this campaign, more than anything else, that gave French Protestantism its persecution complex and Protestant propaganda its paranoid style." This paradoxical sentence raises a question of historical logic that deserves attention from psychohistorians, since its neglect underlies much of the resistance to their faith. My understanding of the phrases "persecution complex" and "paranoid style" is that, insofar as they can be given precise meaning, they assume a comparison of internal with external reality—that is, they are useful only when an independent appraisal of the external situation and the information about it available to the historical actor suggests that the latter has reacted "irrationally" or "excessively," with disproportionate emotion or behavior that in the judgment of observers is not justified by the external situation. In this article Professor Kelley gives evidence that Protestants were being executed by the authorities. Does it make sense, then, to categorize their publicity as displaying a "paranoid style" unless one is willing to adopt a subjectivist and esthetic mode of categorizing human attitudes, one in which a murdered man who had complained of being in danger would still be categorized as paranoid?

There is an important if subtle alteration of meaning involved in this use of words, which underlies the skepticism of many scholars toward psychohistory. Psychiatric concepts and terms often carry an indirect polemical effect because they derive from an intellectual system that is not ethically neutral but loaded in favor of some styles of behavior over others. If the Huguenots displayed a "persecution complex," then an implicit comparison seems to be involved: what kind of attitude and behavior would have been free of persecution complexes and paranoid styles? The implicit assumption may be that some form of stoic accommodation to the situation at hand is the path of wisdom, and therefore of "health." Historians who wish to use psychiatric concepts should be aware of their derivation from a system of interpretation that entails its own value commitments. Articles like Professor Kelley's do us a service by exemplifying this quality of psychohistory.

FRED H. MATTHEWS
York University, Downsview

MR. KELLEY REPLIES:

I agree with Mr. Matthews about some things. I accept his view of "persecution complex" and "paranoid style" (though I prefer Richard

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Hofstadter's, which I had hoped would come to most readers' minds). I, too, deplore an "esthetic mode of categorizing human behavior" (as well as the pejorative use of such a description for problems outside the scope of quantitative methods). I agree that a murdered man has cause for complaint (and no doubt with other propositions on this level of penetration).

Still, I am persuaded that there was a wide gap between the Catholic campaign of denigration and suppression (more specifically, Catholic policy, power, and connections, as known to men like Henry of Navarre and Hotman) and Protestant interpretations thereof (as encouraged by men like Navarre and Hotman). Whether or not Protestant behavior was "justified" (the term is Mr. Matthews's), Protestant ideological creations—theories of diabolical Italianism, universal conspiracies, forgeries—were indeed "excessive" and constituted a kind of political paranoia not uncommon in wars of this intensity. Needless to say, Catholics generated their own sort of irrationality, but I was concerned with Protestant propaganda.

My suspicion is that what disturbs Mr. Matthews is not the logic of this problem, which is tragic rather than paradoxical, but rather the discomforts of dealing with what he is pleased to call "internal reality," which he apparently limits to the immediate emotion of fear. This would at least help to explain why he confuses the problem of Protestant values and "subjectivism" with the problem of mine, which is something else again; I do not exclude the possibility that I am wrong in my estimate of sixteenth-century reality and Protestant perception of it.

I do not really think my article contributes much to the discussion of the interesting and important methodological problems of psychohistory; this sort of obtuse dialectical exercise contributes even less.

DONALD R. KELLEY
Harvard University

TO THE EDITOR:

In her review (*AHR*, 77 [1972]: 1521–24) Joan Hoff Wilson called *Khrushchev and Kennedy in Retrospect* an "intellectual disaster." If so, why was she unable to cite any errors of fact? She also complains of "inadequate research (primarily newspaper accounts and a handful of secondary sources)." One secondary source was cited once; the newspapers include *Pravda, Izvestia, Neues Deutschland,* the New York *Times,* the Washington *Post,* and (indirectly) *Jenmin Jih Pao.*

Statements by U.S. officials and rumors and leaks in the American press were the main sources of information for Soviet policy makers. And Communist news items are especially useful when we ask ourselves what effect they were designed to produce abroad.

Dr. Wilson also criticizes "cold war clichés." Clichés become such because they are apt and descriptive. Any student can judge the final charge for himself ("disjointed and rambling"). Unfortunately his library probably will not have the book. With their limited budgets, librarians at colleges and smaller universities have to depend on reviews when selecting new titles.

Liberal faculty members would never dream of banning or burning books, but unaware of their own ideological bias, they do it quite effectively with reviews of this sort.

JAMES E. MCSHERRY

DR. WILSON REPLIES:

Mr. McSherry is obviously more concerned about sales than the quality of his book. Long before the *AHR* review of his book appeared, it had already been negatively received in the July 1971 issue of *Library Journal* and the October 1971 issue of *Choice,* two standard sources consulted by librarians before they purchase new editions. If libraries have not bought the book by now, I fail to see how my review, appearing almost a year and a half after publication, can be held responsible for poor sales to date.

The charge that the book was an "intellectual disaster" was prefaced by the words "in contrast to the works" I was also reviewing. I must reaffirm that judgment because McSherry's book is clearly inferior to the other two volumes in the area of their common concern. For example, the text and footnotes for the chapter on Cuba do not begin to reveal the complexity and dangers involved in the missile crisis or even refer to some of the secondary works on the formation of Soviet policy cited in McSherry's inadequate twenty-two item bibliog-

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raphy. It is even possible that the author does not understand what a secondary source is since he falsely claims to have cited only one such source. My point is that McSherry consulted and utilized too few of the available primary and secondary sources in what remains a superficial and outdated discussion of the Cuban missile crisis compared to the Allison and FitzSimons accounts.

Moreover, there are no references anywhere in the book to the Oral History Collection at the John F. Kennedy Library, to the important reports of the Institute for Strategic Studies and the Rand Corporation (especially by Roman Kolkowitz and Amron Katz), or to Khrushchev's memoirs, let alone the numerous congressional hearings bearing on the topic of Cuba or other foreign policy events of the Kennedy-Khrushchev years. Scattered newspaper accounts no longer constitute adequate sources for a serious, scholarly diplomatic study of this period.

McSherry's letter also ignores the basic weakness running through the entire book, which my review noted, that is, the absence of any detailed treatment of Kennedy's foreign policy in preference for a superficial summary of Soviet diplomacy from 1957 to 1964. This would of necessity have entailed some consideration of the vast amount of literature involving theories about diplomatic decision making during cold-war crises, Sovietology, and the prolonged test ban negotiations to mention only a few areas of research McSherry ignores entirely.

Finally, I was only too well aware of my own ideological biases as I reviewed this book. I am as opposed to unthinking apologists for Kennedy's foreign policy as I am to unthinking critics of it who idealize brinkmanship and the use of force in American foreign relations. Neither approach provides any means or understanding for pulling out of the cold-warrior syndrome.

JOAN HOFF WILSON
California State University, Sacramento

TO THE EDITOR:

The release of new Soviet documents, perhaps absolving Stalin from inclination toward peace with Germany in 1943, would have been a gratifying, though somewhat embarrassing, response to my article, "Stalin and the Prospect of a Separate Peace in World War II" (*AHR*, 77 [1972]: 1365–88). Instead I received from different quarters a response that is only gratifying, for it supports the thesis expressed in the article and complements it with additional interesting details.

Robert Wolfe of the National Archives has very kindly called my attention to a series of captured German documents that cast further favorable light upon the authenticity of Peter Kleist's postwar claims about peace feelers with Soviet agents in Stockholm during the war. The pertinent documents are in a dossier from the files of the Reich Security Head Office filmed as microcopy T-175, roll 579, frames 122–46. Most important among them is the unsigned "Vermerk betreffend Sowjet-Kontakt," (frames 124–25), dated August 4, 1944, which confirms the essentials of Kleist's assertion about the attempted contacts in September of the previous year (p. 1383 of my article).

The rest of the documentation shows that in the summer of 1944 the same intermediary, Edgar Clauss, again tried to convince Kleist in Stockholm about the seriousness of Soviet intent. Ribbentrop requested Hitler's permission to explore the matter further, but the Führer refused. It is not clear whether Ribbentrop afterward persuaded Hitler to reverse this decision or—more likely—began to act on his own. In any case, he sent Kleist to Stockholm in September with orders to renew the liaison with Clauss. Kleist traveled to the Swedish capital no less than three times in two weeks. It is interesting that in his memoirs he completely ignored these frantic approaches to the Russians—possibly to preserve his reputation in the eyes of the German extreme Right, whose causes he frequently expounded until his suicide in 1972.

Kleist's abortive missions in 1944 were episodes in the story of proliferating German peace efforts during the last year of the Third Reich. While he was in Stockholm in June and July, for example, a member of the anti-Hitler conspiracy, Adam von Trott zu Solz, was also in town, trying in vain to talk to Soviet Ambassador Kollontay. And Kleist's September trips were possibly connected with peace plans of some of the leading SS, described in a 1957 com-

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ment by Alexander Dallin ("Vlasov and Separate Peace: A Note," *Journal of Central European Affairs*, 16 [1957]: 394–96).

As far as the Russians are concerned, however, nothing indicates that their aspirations in the summer of 1944 extended beyond mere collecting of information about the various peace stirrings among the Germans. The evident relief with which Moscow greeted the failure of the July 20 conspiracy shows that Stalin's interest in an accommodation with Germany had long passed by this time. The unique conditions that had seemed to favor such an accommodation a year earlier were never again to be repeated.

VOJTECH MASTNY
Columbia University

TO THE EDITOR:

In my review of Anton Vantuch and L'udovit Holotík (eds.), *Der österreichisch-ungarische Ausgleich 1867 . . .* (*AHR*, 77 [1972]: 1478–80), I misquoted Professor Robert A. Kann. The sentence in question read: "Several of the participants took issue with Kann's conclusion that the compromise ought to be judged 'only according to the purpose of those who created it.' " The direct quotation should have begun with "according." I regret the error.

SOLOMON WANK
Franklin and Marshall College

TO THE EDITOR:

It is perhaps appropriate that a Freudian slip should occur in a paper affecting to treat a psychohistorical question. On the last page of my article, "Martyrs, Myths, and the Massacre: The Background of St. Bartholomew" (*AHR*, 77 [1972]: 1323–42), I applied the term "guild-ridden" to sixteenth-century propaganda and, by implication, to twentieth-century historiography. Suitable as the expression may be in the latter case, the intended epithet was "guilt-ridden."

DONALD R. KELLEY
Harvard University

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Recent Deaths

ROSALIE L. COLIE, internationally celebrated for her work in the cultural history of early modern Europe, drowned on July 7, 1972, when her canoe overturned in the Lieutenant River near her home in Old Lyme, Connecticut. Her years were cut unseasonably short, but in them were compressed many lifetimes of creativity, courage, and generosity.

Professor Colie was born in New York City in 1924, the daughter of Frederic R. and Rosalie L. Colie. She received her B.A. in 1944 from Vassar College, where she studied with Violet Barbour and Mildred Campbell. She went on to Columbia University for her graduate training, working especially with Marjorie Hope Nicolson and Garrett Mattingly, and she obtained an M.A. in 1946 and a Ph.D. in history and English in 1950. Subsequently Professor Colie's research and teaching led her across many borders, geographical and scholarly. She began at Douglass College as an instructor of English and from 1949 to 1961 taught English and humanities at Barnard College. She next joined the department of history at Wesleyan University and then became professor of history and English at the University of Iowa, where she remained until 1966. In the following years she was a visiting professor of English at Yale, a research professor at Lady Margaret Hall, Oxford, and a professor of English at Victoria College in the University of Toronto. In 1969 she was named Nancy Duke Lewis Professor at Brown University and joined the departments of English and comparative literature. At her death she was chairman of the department of comparative literature. Professor Colie also traveled widely during those years, her research taking her to England—where she often visited the Warburg Institute and was closely connected with E. H. Gombrich—to the Netherlands, France, and other parts of Europe. She was a Guggenheim fellow in 1959–60.

The fruits of Professor Colie's research were abundant and were gathered from widely different fields. Whether it was a matter of scientific thought, the poet's craft, or the historian's task, she had a sure grasp of the relevant analytical tools and a store of learning that would have made Robert Burton—one of her favorites—turn to marvel. Of the seven books that she had completed by her death, historians will perhaps know best *"Some Thankfulnesse to Constantine"* (1956), a portrait of the Dutch virtuoso Constantijn Huygens; *Light and Enlightenment* (1957), an elegant and compact study of the thought of the Cambridge Platonists and the Dutch Arminians; and her playful and expansive *Paradoxia Epidemica* (1966). The last was an examination of the paradox, not as a mere literary device but as a central mode of thought and expression in the learned culture of the sixteenth and early seventeenth centuries.

Professor Colie was always conscious of the methods and goals of the cultural historian, from her 1958 essay on " 'Method' and the History of Scientific Ideas" (*Journal of the History of Ideas News Letter*, 4 [1958]: 75–79) to her important essay on "Literature and History" (in James Thorpe, ed., *Relations of Literary Study. Essays on Interdisciplinary Contributions* [1967], 1–27). It was the rules and relations of one's subject that counted—the rules being the rules of style and genre according to which a work was written at a certain time and place; the relations, the precise connections of cultural forms and persons with other forms, persons, and events of the period. It was for his failure to make connections between play and cultural

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style on the one hand and political and economic life on the other that she took Johan Huizinga to task (*AHR*, 69 [1963–64]: 607–30), while she also reproached historians of the *Zeitgeist* for their eagerness to find simple connections everywhere.

A scholar of the humanities, Rosalie Colie was supremely concerned with human beings and the quality of personal relationships. Wherever she traveled, there one can find friends and students whose lives were changed by the teaching and unstinting support that she gave them. Who can forget her lectures—erudite, witty, crammed with meaning and originality, and tempered by her laugh and the pencil bobbing out of her hair bun? As she sought in her work to show the connections between literary circles, so she created in her life circles of colleagues, often from several departments, who met for friendship and discussion. As she sought in her work to show the richness of mixed genres, so she was herself of "mixed kinds": generous, warm, understanding, yet possessed of exacting standards, highly critical and committed to a position when she believed the occasion warranted it. "Multum in parvo" she said of the Renaissance emblem, and "multum in parvo" her friends have said of her, whose life was extinguished even while the bloom was still bright.

NATALIE ZEMON DAVIS
University of California, Berkeley

W. H. B. Court died at Birmingham, England, on September 30, 1971. Born in 1904 at Cirencester, the son of a bookseller, he was educated at Newbury Grammar School and the University of Cambridge. An admiration for R. H. Tawney led him after graduation to an interest in the economic policy of the Calvinistic colonizers of New England. In 1927 he went to Harvard to research this subject under the direction of S. E. Morison. Court also greatly benefited while there from the advice and help of Edwin Gay. Two years later Court accepted a lectureship in economic history in the Faculty of Commerce of the University of Birmingham. In 1947 the university invited Court to fill a newly created chair of economic history, and subsequently he became the first head of a new department of economic and social history.

The Birmingham environment turned Court's interest toward industrial history, and in 1938 he published *The Rise of Midland Industries, 1600–1838*, a classic study of regional industrial development. The close friendship and stimulus of W. K. Hancock at Birmingham had begun to move Court in the direction of imperial economic history when the war intervened. He soon found himself in government service at the Ministry of Shipping. In 1943 Hancock recruited him into the cabinet office to assist in the preparation of the official history of the war. Court himself wrote the volume that appeared in 1951 on the coal industry during the war.

After the war the Cambridge University Press invited Court to complete the *Concise Economic History of Britain*, which was left unfinished by the death of Sir John Clapham. The continuation developed into a separate volume covering the period since 1750 and was published in 1954. It is still one of the most widely read introductions to British economic history.

From 1956 to 1959 Court occupied the deanship of Birmingham's Faculty of Commerce, an office that left little time for writing and research. His release from the heavy demands of university administration enabled him to publish in 1965 his *British Economic History 1870–1914: Commentary and Documents*, a work of impressive originality and distinction. At the time of his death he had made good progress on two further works, an edited collection of papers by R. H. Tawney, including an assessment of his life and work, and a major study of the economic history of Britain during the First World War. Court had already outlined his approach to the latter theme in one of the papers he brought together and published in 1970 under the title *Scarcity and Choice in History*. The collection also includes a remarkable autobiographical essay, "Growing Up in an Age of Anxiety," which sensitively describes the experiences and influences that shaped his outlook as a historian.

In recognition of his stature as a scholar Court was elected a Fellow of the British Academy and an Honorary Fellow of Downing, his old Cambridge college. With Sir William Ashley, the first dean of Birmingham's Faculty of Commerce, Court shared the distinction of being the only scholar from a British university

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apart from Cambridge and London to have been elected president of the Economic History Society. Ashley and Court both died in this office at very nearly the same age. Except for a brief interlude between 1925 and 1929, their careers in Birmingham's Faculty of Commerce cover the entire period of its first seventy years.

His many friends the world over will remember W. H. B. Court for his unconscious personal dignity, his humility, gentleness, and total integrity, and for the strength of his loyalty and the depth of his consideration for others. He served the world of learning not only in all that he did, but in being what he was. In commemoration of him as a scholar and as a man, the University of Birmingham, where he taught and worked for over forty years, has established the W. H. B. Court Memorial Prize for the best undergraduate degree result in economic and social history.

DAVID WIGHTMAN
University of Birmingham

CHARLES DESMOND HART, associate professor of history and a member of the York faculty since 1965, died in Toronto on September 18, 1972, after a long illness. He had been on sick leave since April 1, 1971. Those colleagues who worked with him over a period of years know that the dimension of their loss is not easily measured. Des Hart was a devoted scholar and teacher, an unfailingly helpful colleague, and a decent human being.

Des was born on June 8, 1935, in Cornwall, Ontario. After completing his education at local schools he spent a period teaching for the Department of Citizenship and Immigration. He took a B.A. at Queen's University in 1960 and did graduate work with G. M. Craig and J. T. Saywell at the University of Toronto. Des pursued his passionate interest in American history and literature in the Civil War period at the University of Washington in Seattle. His work with T. J. Pressley led to a dissertation on "Congressmen and the Expansion of Slavery into the Territories: A Study in Attitudes, 1846–1861" (1965). While at Washington he taught American history and was involved in counseling undergraduates. He also served as visiting lecturer at Queen's University in 1963. Upon completion of his dissertation he was appointed assistant professor at York, and he was promoted to the rank of associate professor in 1971. In 1970 he married Brenda L. Ough, and he leaves his wife and an adopted daughter, Heidi.

Des Hart made an important contribution to York's history program, especially at the university's newly established main campus. As a pioneer member of a new department he helped lay out the sequence of courses in his field. He was director of the large team-taught introduction to American history course until his illness in 1971. He also collaborated with Don Summerhayes of the English department on York's first interdisciplinary course in American civilization. At the onset of his illness he had published several articles on Civil War history. One of his sharpest regrets was the incomplete state of his extensive research and writing on the slavery issue. His considerateness and gentleness as a human being and his deep professional commitment to scholarship will long be remembered by those colleagues and students who had the good fortune to know him.

MARC EGNAL
York University, Downsview

THOMAS FRANK JAMES KENDRICK died Easter day, April 2, 1972, in Mineola, New York, cut off from a promising career as a historian of eighteenth-century England. He is survived by his wife and his mother.

Born in Philadelphia on September 9, 1926, the son of a Pennsylvania manufacturer, Dr. Kendrick had originally intended to follow a business career and received a B.S. in economics at the Wharton School of the University of Pennsylvania. A sincere love of history, however, turned his path toward scholarship, and he earned a B.A. with honors at Cambridge University in 1952 and an M.A. in 1957. He received his Ph.D. from the University of Toronto in 1961. His thesis was entitled "Religion and Politics in Early Georgian England." After a series of teaching positions Dr. Kendrick finally settled permanently at Queens College of the City University of New York and gave that institution fourteen years of brilliant teaching and devoted service.

Dr. Kendrick was an untiring scholar. His passion in life was to know his chosen field thoroughly in all of its ramifications; he hesitated to rush into print unless his work

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was a worthy contribution to history. A last article, "Sir Robert Walpole, the Old Whigs and the Bishops, 1733–1736: A Study in 18th Century Parliamentary Politics" (*Historical Review,* 11 [1968]: 421–45), manifested a solid command of the materials in his chosen field of endeavor.

A person of sincere religious convictions, Dr. Kendrick was earnestly desirous of investigating the history of the English church. His own contribution was to be a definitive biography of Bishop Benjamin Hoadly, the controversial eighteenth-century English primate. Dr. Kendrick pursued the scent of original materials wherever it led to garner every single grain of knowledge—through dusty libraries in Great Britain, in every nook and cranny at the old bishop's several dioceses of Bangor, Hereford, Salisbury, and Winchester—leaving no obscure stone unturned, untying every bundle of possible historical data, and flipping through miles of index cards. His research was completed and the task of writing had begun when death claimed him.

Looking through the vast personal library that Dr. Kendrick accumulated skillfully after years of search and selection, one is impressed with the breadth and depth of his knowledge as well as his keen understanding of the printed material of English and Canadian history. His knowledge of bibliography was unusually broad and detailed. His honest affection for books was one of his finest characteristics.

Dr. Kendrick's real talent was exhibited in his brilliant classroom teaching—a talent, he believed, that must be recognized as the primary concern of the modern punch-card university if it is to survive in future good health and continue to prosper. Only his closest friends were aware that his excellent teaching was achieved only after difficult years of struggling and conquering a speech impediment that would have defeated one with less will to succeed. He put his entire heart and soul—and even his body—into his lecturing. He emerged each day from his classroom with his clothing literally covered with chalk dust, his blackboard turned white with writing from window to door.

Dr. Kendrick's colleagues, in sorrow, have established a library fund in his memory for the purchase of books on Canadian history to carry out one of his cherished projects. Persons interested in contributing a permanent memorial may forward their checks to the Thomas F. J. Kendrick memorial fund, Paul Klapper Library, Queens College, Flushing, New York.

WILLIAM J. CHUTE
Queens College,
City University of New York

ROY FRANKLIN NICHOLS was not only a gifted historian in the older tradition of carefully researched and finely written narrative, but one of the leaders of his generation in bringing political history closer to the social sciences. Serving as representative of the American Historical Association on the board of the Social Science Research Council from 1935 through 1956, on its guiding Problems and Policy Committee during most of this period, and as chairman of the board from 1949 to 1953, he was responsible for a large part of the program of the council in the field of history. This included sponsoring many conferences and monographs as well as the council's *Bulletins 54* (1946) and *64* (1954) on historiography. He participated actively in both the planning and writing of the council's *Generalization in History* (1963).

His academic career was a model of the rapid completion of graduate work and early promotion wished for by all young historians. Born March 3, 1896, in Newark, New Jersey, he took an A.B. from Rutgers in 1918, an M.A. in 1919, and completed his Ph.D. at Columbia in 1923. After teaching at Columbia from 1922 until 1925 he went to the University of Pennsylvania as assistant professor and, in 1930, became a full professor.

Meanwhile, in 1920, he married Jeannette Paddock, who also received her doctorate in history from Columbia in 1923. For the next half century they were a partnership working continuously on historical projects both jointly and separately. Late in life Roy Nichols said that "a common historical interest has produced a 'career' which belongs to us both and cannot really be thought of in two parts." Jeannette Nichols has published many articles that represent strictly her own interests, but mutual contributions were seldom absent from their work. Eventually Jeannette Nichols, whose personal interests are primarily in economics and finance, was drawn into the history department of the University of Pennsylvania, first as an associate

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professor teaching American economic history and from 1960 to 1964 as chairman of the graduate program in economic history. Had ill health not intervened late in that decade, a joint biography of John P. Sherman would have been published. They shared the title of historian of the university, and Jeannette Nichols is continuing work on a history of the University of Pennsylvania since the beginning of World War II.

In his major historical writing Roy Nichols sought to bring the philosophy more than the specific methods of political scientists and social psychologists to bear upon history. His doctoral thesis was *The Democratic Machine, 1850–1854*, published the year he received his degree. Unusually mature for a first book, it initiated a long-range project on the nature of democracy and its party politics from 1850 to the outbreak of the Civil War. The second volume, *Franklin Pierce*, appeared in 1931 and the concluding volume, *The Disruption of American Democracy* (1948), was awarded the Pulitzer Prize for history in 1949. Turning back to earlier origins of the causes for the breakdown of the democratic process in 1861 he wrote a series of studies relating political institutions to particular American characteristics: *Advance Agents of American Destiny* (1956); *Religion and American Democracy* (1959); *The Stakes of Power, 1845–1877* (1961); and *Blue Prints for Leviathan, American Style* (1963). His final major publication was the autobiographical *An Historian's Progress* (1968) in which he treated the events of his distinguished career with the detached objectivity of the trained historian. During the entire period of the writing of these last five books he was also a major figure in the administration of the university. Had his final illness not prevented it, he would have been editor of a history of the city of Philadelphia, which is now being completed by others.

Nichols's gift for writing and felicitous, witty expression made him much in demand as a teacher and lecturer. He taught as a visitor at Columbia (1944–45) and Stanford (1952) and in 1948–49 held the Pitt professorship and received an M.A. at the University of Cambridge. As late as 1962 he and Jeannette Nichols accepted the rigors of serving as lecturers in India and Japan. Following an L.H.D. from Rutgers in 1941, so many colleges and universities awarded him honorary degrees—nine in all—that the late-comers were hard put to find a title he did not already possess.

Believing that our neglected social history had to be recovered at the grass-roots level, he imparted a new vigor to the study of state and local history in Pennsylvania. As early as 1932–33 he was selected president of the Middle States Association of History Teachers. In the latter of these two years he was elected to the board of the Historical Society of Pennsylvania, one of the oldest and most respected societies in the nation, of which he was honorary vice-president at the time of his death. He was instrumental in organizing the Pennsylvania Historical Association and served as its president from 1936 to 1939. More than a decade of work with the Federation of Pennsylvania Historical Societies led him to its presidency for 1940–42. At the same time he worked as a member of the commission planning the bicentennial of the University of Pennsylvania and as a member of the Historical Commission of the Commonwealth. From 1946 to 1957 he was president of the Genealogical Society of Pennsylvania. In 1960 he was named to the new Philadelphia Historical Commission, which he chaired from 1967 until his resignation because of ill health in 1970. Meanwhile, he served as director of the Philadelphia Atheneum from 1956 on and as its vice-president in 1966. In 1962 he received its Haney medal for his book *The Stakes of Power*. Governor Shafer's committee of 100,000 Pennsylvanians gave him an award of excellence in 1967. In honor of his services to the history of the state, as well as of his distinction in the nation, *Pennsylvania History* devoted the entire January 1971 issue to his career and writing.

His achievements of national scope also brought him many honors. In 1945 he was elected to the American Philosophical Society and from 1953 to 1969 he represented the society as chairman of the board of the Benjamin Franklin papers. For a decade (1958–68) he served on the board of the American Academy of Political and Social Sciences. A member of numerous committees of the American Historical Association going back to 1925, he was elected to the council in 1943, to the vice-presidency in 1965, and to the presidency in 1966. He was a senator of Phi Beta Kappa

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after 1961 as well as a director or officer of numerous other honorary and learned societies. In 1969 he was made honorary consultant in American history to the Library of Congress.

Even-tempered, genial, and efficient in carrying out responsibilities, he was widely in demand for administrative services. An important trustee of Rutgers from 1944 on, he became a member of its Board of Governors in 1958. The most important use of Nichols's administrative abilities, however, was at the University of Pennsylvania. In 1952 he became dean of the Graduate School of Arts and Sciences and the following year accepted the additional title of vice-provost. As a member of the highest staff committees he participated in most of the important decisions of the university. His unusual services to higher education were recognized in the presidency of the Association of Graduate Schools in 1964 and the chairmanship of the Council of Graduate Schools of the United States in 1965. His many positions and natural eloquence led to severe demands on his time and energy in representing the university at formal occasions. Accounting for his ability to do so much Nichols said that his work was a hobby and a recreation as well as a vocation. After his retirement as emeritus professor in 1966, the university named its graduate residence hall the Nichols Building in memory of the "distinguished services" of both the Nicholses.

He died in Philadelphia on January 11, 1973, survived by no close relatives except Jeannette Nichols, who hopes to complete their unfinished tasks. Friends wishing to commemorate him may contribute to the Nichols Fund for Historical Projects at the University of Pennsylvania, Philadelphia, Pennsylvania 19104.

THOMAS C. COCHRAN
University of Pennsylvania

HENRY CLAY REED, emeritus professor of history at the University of Delaware, died suddenly on July 29, 1972, at his home in Newark, Delaware. Born in Tyrone, Pennsylvania, on May 15, 1899, Reed took his B.A. degree at Bucknell, his M.A. at Pennsylvania State, and his Ph.D. at Princeton, where he wrote a dissertation under the direction of Thomas J. Wertenbaker on crime and punishment in New Jersey. Before his retirement in 1964, Reed had taught for forty years at the University of Delaware.

His most notable publication is probably *The Burlington Court Book: A Record of Quaker Jurisprudence in West New Jersey, 1680–1709,* of which he was coeditor and which was volume 5 of the American Legal Records series published with the assistance of the Littleton-Griswold Fund by the American Historical Association (1944). He is best known as editor of *Delaware: A History of the First State* (1947), and he was also the editor of *Readings in Delaware History: Economic Development* (1934) and, with his wife, of *A Bibliography of Delaware through 1960* (1966). He also collaborated in the editing and translation of Charles de Lannoy's *History of Swedish Colonial Expansion* (1938). His last book, primarily for children, was *The Delaware Colony* (1970).

Through the years Professor Reed wrote many articles, mainly on local history, but some of importance transcending the local scene, particularly his "The Delaware Constitution of 1776" (*Delaware Notes,* 6 [1930]: 1–42), "Lincoln's Compensated Emancipation Plan and Its Relation to Delaware" (*Delaware Notes,* 7 [1931]: 27–78), "The Early New Castle Court" *Delaware History,* 4 [1951]: 227–45), and "The Court Records of the Delaware Valley" (*William and Mary Quarterly,* 4 [1947]: 192–202). His careful notes for these and other studies, including his unfinished work on the history of counterfeiting in the United States, are being deposited in the library of the University of Delaware for the use of other scholars.

For almost forty years Professor Reed suffered from diabetes and had been required to regulate his life very carefully, particularly in regard to diet and exercise. Nevertheless he taught full schedules, served on most of the important committees in his university, took his share of administrative assignments, and was active in a number of state organizations, including the Archaeological Society, the Swedish Colonial Society, and the Historical Society of Delaware, in which to the time of his death he was an active member of the committee on publications. He played an important role in the reorganization of the Delaware State Archives and in the Delaware Tercentennial Celebration of 1938. From 1944 to 1952 he was chairman of the department of history. He

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served briefly thereafter as the first chairman of the American Studies program at Delaware and was one of the founders of the Winterthur Program in Early American Culture, which operated in conjunction with the Henry Francis du Pont Winterthur Museum.

A quiet, modest, gentle man, though firm in his convictions, he was highly esteemed by those who knew him for his dry wit, his independent judgment, his devotion to scholarship, and his facile literary style.

JOHN A. MUNROE
University of Delaware

With the death on May 27, 1972, in Chapel Hill, North Carolina, of JOHN LESLIE SNELL, JR., the world of German history lost one of its most dedicated and illustrious students and the American Historical Association one of its most enthusiastic and generous workers. Born in Plymouth, North Carolina, on June 2, 1923, he entered the University of North Carolina in September 1940, just as the Rome-Berlin-Tokyo alignment took firm shape. Early in his junior year he joined the Army Air Corps, and it was as a bomber pilot that he got his first view of Europe. He returned to the university at Chapel Hill in June 1945 with a strong desire to study and teach European history. Superbly disciplined, sure of his goal, and willing to work long hours, he completed requirements for the A.B. in 1946, the M.A. in 1947, and the Ph.D. in 1949.

Professor Snell's teaching career, though relatively short because of his untimely death, was nevertheless rich and varied. He taught two years (1949–51) at Wichita University; thirteen years (1953–66) at Tulane University; two years (1966–68) at the University of Pennsylvania; and the last four years of his life at his Alma Mater, the University of North Carolina, as University Distinguished Professor. He also did summer teaching in four universities: Tennessee (1947); Michigan (1953); Vanderbilt (1954); and Stanford (1963).

Professor Snell conceived the role of the teacher of history broadly, and the twenty-three years of his teaching career were replete with solid accomplishments not only in the classroom but at the writing desk, on university committees, and in scholarly societies and professional associations. As a teacher Snell was forever challenging his students—undergraduates as well as graduates—to perform to the limit of their capacities, and he almost invariably covered the papers of even his best students with suggestions as to how they could do better. Equally talented as a writer and convinced that research and writing were essential to creative teaching, he published five books, three text anthologies, thirty-odd research articles, and left a manuscript nearly ready for the press. Perhaps the publication that gave him most satisfaction was *The Meaning of Yalta* (1956), which was done in collaboration with three historians whom he admired: Charles Delzell, Forrest Pogue, and George Lensen. German historiography will be a little poorer because Professor Snell did not live to finish his projected two-volume work on democracy in Germany (vol. 1, *Century of Frustration, 1789–1914* and vol. 2, *Birth of the Weimar Republic, 1914–1918*). Fortunately he had the first ten chapters of the manuscript nearly finished, and Professor Hans Schmitt, a former colleague at Tulane, has graciously undertaken to prepare this part of the manuscript for publication.

Professor Snell was also quite active both in the internal affairs of the universities where he taught and in scholarly and professional associations. At Tulane, for example, he not only served on a variety of committees, including the president's advisory committee, but he served as dean of the graduate school of arts and sciences during his last three years there. But, perhaps next to the classroom, Professor Snell's most effective work was done in scholarly and professional associations. He gave countless hours of devoted service to both the American and Southern Historical Associations. Between 1955 and 1965 in the AHA, for example, he served on the council, the committee on nominations, the committee on graduate study, the committee on Ph.D. programs, the committee for the study of war documents, the committee for the George Louis Beer Prize, the committee on scholarly historical resources, and he was secretary of the modern European historical section and chairman of the conference group for Central European history. In the same ten years he participated in six panels at annual meetings. Even more important, perhaps, he directed the study on the education of historians that the association launched in 1958

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and indeed wrote the report, *The Education of Historians in the United States* (1962). We shall not recount his many services to the SHA, other than to say that he helped found its European section and served it both as secretary-treasurer and as chairman.

But absorbed as Professor Snell was in teaching, writing, and the work of scholarly and professional bodies, he found time for his family and for a large circle of friends, which included many German scholars. Indeed he was a warm and gracious person who worked relentlessly but joyfully at the endless task of being both a better scholar and a better man.

Professor Snell is survived by his widow, Mrs. Maxine Pybas Snell of Chapel Hill, N.C., and three children: Mrs. Marcia Snell Pantell, John McCullough Snell, and Miss Leslie Ann Snell. There is a John Leslie Snell, Jr., Memorial Fund for the purchase of books on German history that is administered by the Wilson Library of the University of North Carolina at Chapel Hill.

CARL HAMILTON PEGG
University of North Carolina, Chapel Hill

RAYMOND JAMES SONTAG, one of the country's most-respected historians of European diplomacy, died in Berkeley, California, on October 27, 1972, at the age of seventy-four. A native of Chicago, he attended the University of Illinois and completed his Ph.D. at the University of Pennsylvania in 1924 under the supervision of William E. Lingelbach. For the next seventeen years Sontag taught at Princeton University, becoming Henry Charles Lea Professor of History in 1939 and serving as department chairman from 1939 until 1941. In 1941 he moved to the University of California at Berkeley, where he was Sidney Hellman Ehrman Professor of European History until his retirement in 1965. A strong believer in the ideal of public service, he was a member of the Board of National Estimates of the Central Intelligence Agency from 1951 to 1953 and, in later years, consultant to various government agencies. He received honorary degrees from the University of California, Marquette, and Notre Dame, was a member of the American Philosophical Society since 1949, and was president of the Pacific Coast Branch of the American Historical Association in 1959.

Raymond Sontag's first major work, and perhaps his most influential one, was *European Diplomatic History, 1871–1932*, which appeared in 1933. A beautifully articulated and economically written account of the great-power rivalries that led to the First World War, it was characterized by a determined effort to cut through the much-controverted debate over war guilt and to do justice to all of the parties to the debacle. In his view the responsibility for the war was general, caused by the victory of nationalism over the kind of realistic diplomacy that had been practiced by the two statesmen he admired the most, Bismarck, who had known how to use power with restraint, and Salisbury, always "shrewd, patient, never permitting details to obscure the whole picture, seldom mistaking desires for realities, keenly conscious of the sordid frailties of humanity."

The passage of the years has made so much archival material available to researchers that the chapters of Sontag's *Diplomatic History* that deal with the period after 1914 are now inadequate, although the earlier sections remain an excellent introduction to the subject. Time has not affected Sontag's second book in the same way. *Germany and England: Background of Conflict, 1848–1894*, published in 1938, is the most satisfying account that we possess of the way in which British insularity and ignorance and German inferiority and resentment estranged two peoples who might, in the absence of such psychic tensions, have been natural allies. The book is filled with striking vignettes of Cobden, Palmerston, Disraeli, and that rabid tub-thumper Heinrich von Treitschke, and includes, among other fine things, a brilliant analysis of the effects of imperialism in the two countries.

Sontag's last work, published in 1971, was his long-awaited contribution to the Rise of Modern Europe series, *A Broken World, 1919–1939*. In the preface he described his purpose as being to attempt to see this segment of the past as a historian without, however, "losing the fresh, if defective, vision of the contemporary world." It is, indeed, the distinctive feature of the book that it succeeds in capturing the mood of the period it deals with, years in which many young people ("From the dark days

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of the thirties," the author writes, "there is the memory particularly of young people in Paris, or Rome, or Vienna, or Munich, or London.") felt that the world of European thought, feeling, and action was broken but not broken beyond repair, in which men in every field of activity were seeking for solutions to this predicament, and in which tremendous resources of intellectual vigor and imagination were manifest on every side, only to be rendered useless in the end by the inability of the statesmen and politicians to show similar energy and spirit in dealing with the threat of totalitarian nationalism.

In *A Broken World* Raymond Sontag gave to apprentice historians an important example of how effective diplomatic history can be when it is written within the full context of its time and with an awareness of the reciprocal relationship that exists between foreign and domestic affairs. In addition to this they owe him a further debt. After the Second World War, when the British, French, and American governments undertook to publish materials from the captured files of the German Foreign Ministry and the Reich Chancellery, he became the first American editor-in-chief, serving from 1946 to 1949 and exerting great influence on the formulation of the principles that guided the publication of the indispensable series of *Documents on German Foreign Policy, 1918–1945* (1949–). His separate volume, *Nazi-Soviet Relations, 1939–1941*, edited in collaboration with James Stuart Beddie in 1948, provided the first basis for a realistic appraisal of the origins, the troubled course, and the eventual rupture of the alliance between Moscow and Berlin.

While Raymond Sontag's published works will continue to be regarded as models of how diplomatic history should be written, the thousands of persons who had an opportunity to attend his classes will remember him primarily as a teacher. He was a superb lecturer, whose ruminative style engaged students in the problems being discussed so effectively that one junior in the old History 317 course at Princeton in the thirties often had the illusion that he was listening, not to Sontag, but to Bismarck charting his course from the Danish to the Austrian war or seeking to unravel the complexity of the Near Eastern question. It was in small-group discussions, however, that he was at his best, ever attentive to his students' opinions but always skeptical and questioning, forcing them to re-examine their assumptions, guiding them to deeper levels of understanding. His affection for his students was deep and abiding (he once explained to a young historian who was quizzing him about the art of teaching that it consisted almost entirely of liking students), and during the Vietnam years, when relations between many university teachers and their students became strained to the breaking point, he strove patiently and sympathetically to understand the effect of the war upon the present college generation and to appreciate the bitterness and frustration that it brought to its members. His students responded by crowding his classes and seeking him out for private advice and guidance. Although he became an emeritus professor in 1965, no one expected him to stop teaching; nor did he, until death forced him to.

He was a teacher of his colleagues, too, and many of them remember his counsel with gratitude. A former Princeton colleague wrote after his death: "He knew how to help us out by his advice and his example. He widened our interests and our understanding of history by those long evenings of conversation at his house. He could be provocative, and at times wrongheaded, but you never left him without feeling that you had had to think about problems that you hadn't considered before."

GORDON A. CRAIG
Stanford University

The sudden death on June 13, 1972, of ROBERT TOWNLEY TURNER in Missoula, Montana, stunned his many friends. Just approaching the age of fifty-five, he had served at the University of Montana as chairman of the history department, then as dean of the College of Arts and Sciences, and for many years with distinction as director of the university museum, one of the finest small museums in the West. He had spent the entire thirty years of his teaching career at Montana.

To those who might charge him with publishing little, Turner would have pled "O mea culpa." He had the good sense to know when not to publish because he had little to say, or because too much had already been said, or because there was nothing for anyone to say.

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The few bits and pieces he did publish were gems of reflection and particularly of expression.

I remember Bob Turner as one of the few great teachers I have known. I would rank him with Dwight Dumond and the late R. J. Kerner in the classroom, and both would have applauded his performance. His genius for probing, for enunciation, and for detecting lazy or weak work was rare. His lectures in European diplomacy, and particularly those in "Modern War and Western Society," will not soon be forgotten by those who came to hear them.

MELVIN C. WREN
University of Toledo

Other members of the association who have died recently include: Victor L. Albjerg of Purdue University in West Lafayette, Indiana; Frank E. Bailey of Gambier, Ohio; Jane Schwartz Benjamin of Cabrini College in Radnor, Pennsylvania; E. M. Burns of Santa Barbara, California; O. L. Clark of Edwardsville, Illinois; Robert I. Cooper of Brooklyn, New York; L.M. Dambrosio of Brooklyn, New York; L. E. Grove of Washington, D.C.; Herman Hailperin of Duquesne University in Pittsburgh, Pennsylvania; N. A. Holsten of Augusta, Georgia; E. S. Izzo of Peabody, Massachusetts; Gustav E. Johnson of Beloit College in Beloit, Wisconsin; R. W. Leach of Denton, Texas; R. Matamoros of Huntington Park, California; Eugen Rosenstock-Huessy of Dartmouth College in Hanover, New Hampshire; Chester A. Smith of Peekskill, New York; and A. S. Williamson of St. Paul, Minnesota.

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Association Notes

Readers with an eye for editorial detail will have noticed a number of changes on the title page of the *AHR* in recent issues. Beginning with this volume Mr. Webb, formerly managing editor, has been transmuted into mere editor, and the former associate editor, Miss Lane, has assumed the title of managing editor. These changes reflect more accurately the duties attached to the two offices, inasmuch as Miss Lane is responsible for all the day-to-day operations of bringing out an issue of the *AHR* and truly manages the operation, just as one of the original board of editors in 1895 and for many years after managed the operation. These changes bring the *AHR* out of a historical past and into the present realities of the publishing world.

In this issue a number of new names appear on the title page and others have disappeared or will soon do so. Miss Esten Hardee has moved from her post as editorial assistant to superintend the operation of the professional register and the publication of the *Employment Information Bulletin* for the AHA. By the time the issue appears Miss Joan Ansheles will have left the staff of the *AHR* for Wisconsin, although, as she remained responsible for the coordination of the article lists in this issue, her name continues to appear. Miss Marcia Castaneda has moved from the membership department of the AHA to become an editorial assistant working directly with the managing editor. Mrs. Janet Hearne, formerly on the staff of the Booker T. Washington Papers, has joined the *AHR* staff as assistant editor; she will succeed largely to the editorial responsibilities for reviews and back matter that have fallen to Miss Robin E. Byrnes, who will be working more extensively in the bibliographical operation of the *AHR* in place of Miss Ansheles.

In this issue, for the first time in twenty-three years, the list of recently published articles for United States history will not have been prepared by Dr. Wood Gray, who recently retired as professor of history at George Washington University. The readers of the *AHR*, members of the AHA, and American historians generally owe a profound debt to Dr. Gray for his immense labors over so long a period to bring order and visibility to the extensive and continually growing bibliography in a field that few scholars would have tried to encompass singlehandedly. And those of us at the *AHR* will gratefully remember his efficiency, his determination, and his wit and kindness. From now on the article lists in United States history will be prepared in the *AHR* offices under the supervision of Mr. Papenfuse. We expect that through mechanization the material thus compiled can be incorporated into larger bibliographical operations we have in view.

For the academic year 1973–74, Mr. Webb will be on sabbatical leave from the editorship. His place will be taken by Thomas C. Cochran, Benjamin Franklin Professor Emeritus at the University of Pennsylvania, former president of the AHA, and presently visiting professor for the spring semester at the University of North Carolina, Charlotte. The AHA is fortunate to have secured on a visiting basis a scholar who combines broad historical sympathies, a wide knowledge of the profession, and long experience with AHA affairs.

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Festschriften and Miscellanies

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

CORDIER, ANDREW W., editor. *Columbia Essays in International Affairs.* Vol. 7, *The Dean's Papers, 1971.* New York: Columbia University Press. 1972. Pp. viii, 261. $10.00.

DAVID A. J. MACEY, The Peasantry, the Agrarian Problem, and the Revolution of 1905–1907. MARIE L. ROCCA, The Negro in Colombia: An Historical Geography. NORMAN LOUIS CIGAR, The Soviet View of the Egyptian Agrarian Reform: 1958–1963. FELICE D. GAER, A Cybernetic Reform Model for the Soviet Union. MICHAEL J. BUCUVALAS, The Breakdown of a Political System Experiencing Economic Development: Greece, 1950–1967. MICHAEL K. BLAKER, Japanese Foreign Policy Decisionmaking in the Middle Taishō Period: An Institutional Case Study of the *Gaikō Chōsa Kai.* STEPHEN ADLER, The Cuban Missile Crisis: Strategic Theory in Practice. MARGARET ROFF, Disintegration and Integration in Southeast Asia. JAMES B. STEPANEK, The Political Economy of Land Reform.

HEINEN, ERNST, and SCHOEPS, HANS JULIUS, editors. *Geschichte in der Gegenwart: Festschrift für Kurt Kluxen zu seinem 60. Geburtstag.* Paderborn: Ferdinand Schöningh. 1972. Pp. x, 339. DM 38.

Methoden—Kategorien: WERNER GOEZ, Die Anfänge der historischen Methoden—Reflexion im italienischen Humanismus. ALFRED VOIGT, Merkmale der Rechtswissenschaft.

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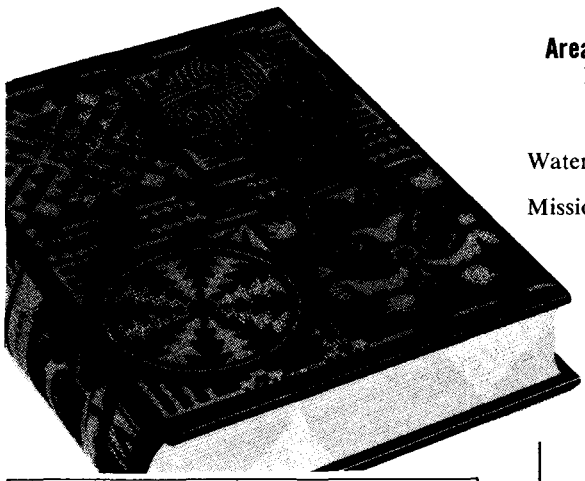
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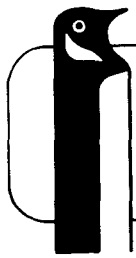
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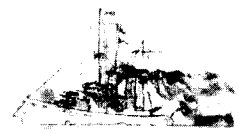
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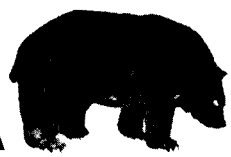




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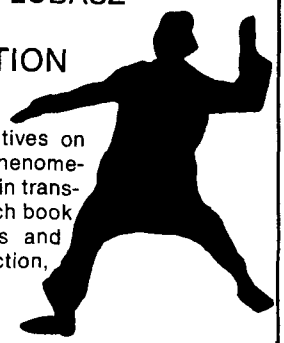
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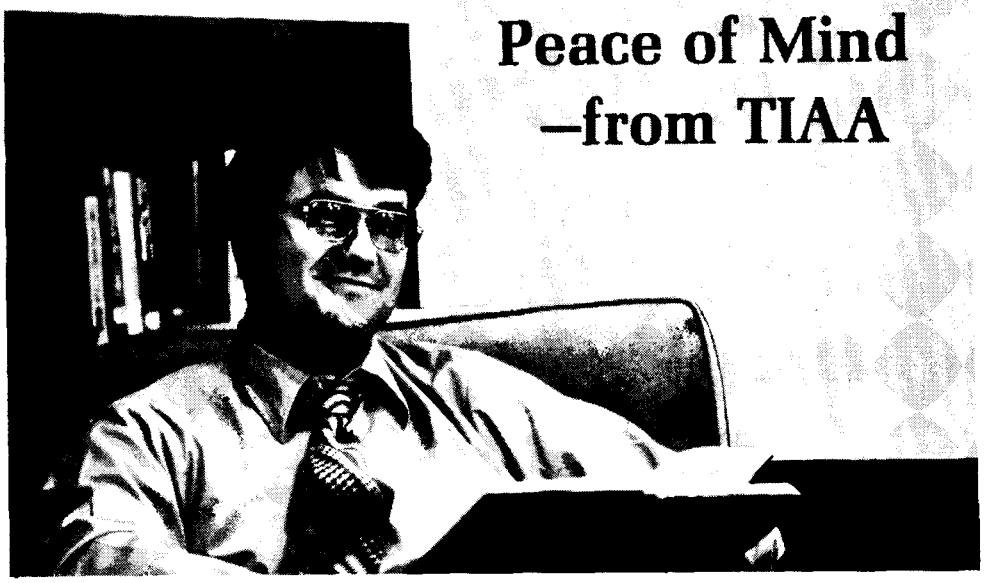
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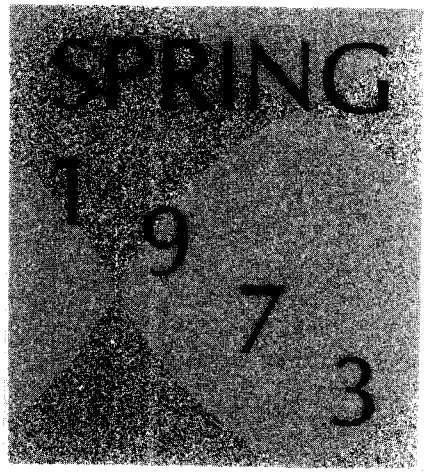
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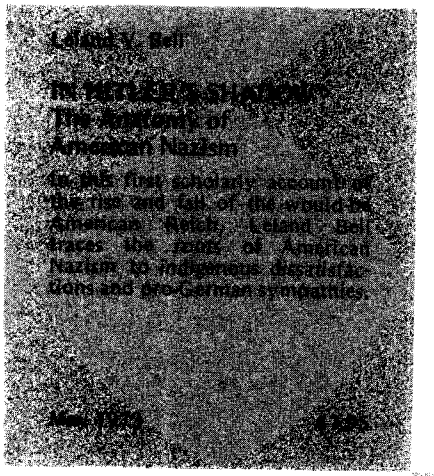
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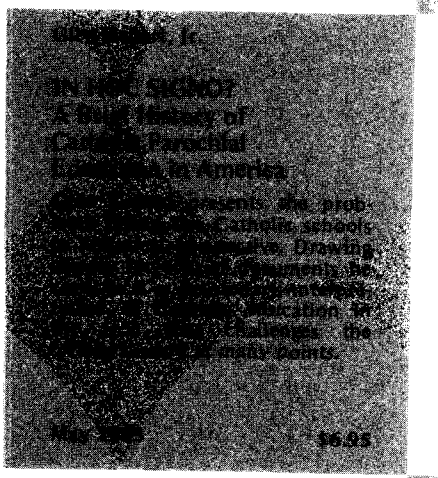
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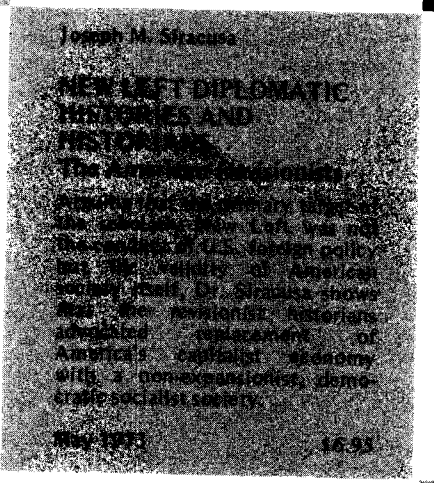
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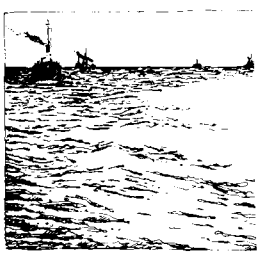
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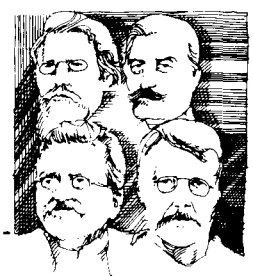
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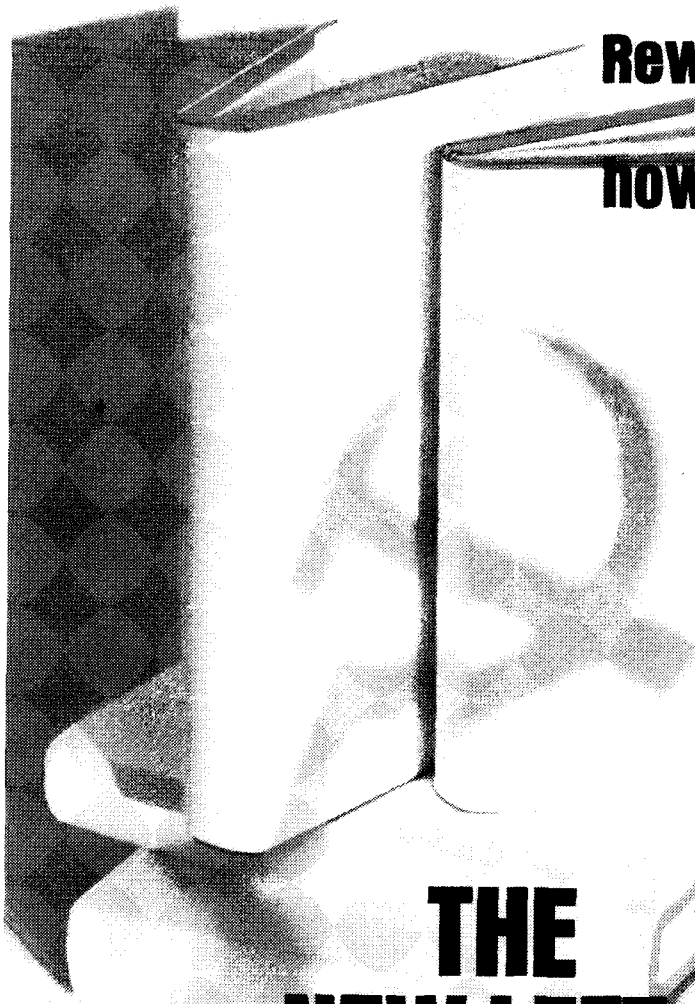
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